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MEDICAL REPORTS,

ON THE

EFFECTS OF WATER,

COLD AND WARM,

AS A REMEDY IN

FEVER AND OTHER DISEASES,

Whether applied to the Surface of the Body, or used Internally,

VOL. I.

INCLUDING

AN INQUIRY INTO THE CIRCUMSTANCES THAT RENDER COLD
DRINK, OR THE COLD BATH, DANGEROUS IN HEALTH.

TO WHICH ARE ADDED,

OBSERVATIONS ON THE NATURE OF FEVER;

AND ON THE EFFECTS OF

Opium, Alcohol, and Inanition.

THE FOURTH EDITION, CORRECTED AND ENLARGED.

By JAMES CURRIE, M. D. F. R. S.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

*Intentiones operationum, quas proposuimus (ut arbitramur) verissima sunt, remedia
intentionibus fida: * * * Rem ipsam experimentum et comprobavit et promovebit.
* * * Opera consilii cujusque prudentioris, sunt effectu admiranda, ordine
quoque egregia, modi faciendi-tanquam vulgaria.*

BACON. *Historia vitæ et mortis.*

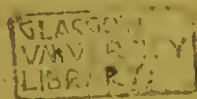
L O N D O N :

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1805.

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1723-1881



TO
FIELD MARSHAL
HIS ROYAL HIGHNESS
THE DUKE OF YORK,

Commander in Chief, &c. &c. &c.

S I R,

THE permission which I have received to dedicate this enlarged edition of these volumes to your Royal Highness, commands not merely my sincere gratitude, but my profound respect. They contain the details of a method of cure in febrile diseases in a great measure new, and which being particularly adapted to the medical practice of our armies, has been honoured with the notice of your Royal Highness. Under the sanction of your name and authority, this publication will obtain the general attention of our military practitioners, its merits will be fairly appreciated, and its usefulness, if established, be widely extended.

At a season when the profession of Arms acquires an importance unexampled perhaps in the
§ *annals*

annals of mankind, whatever may contribute to the life and health of the defenders of their country, rises proportionably in value. In this point of view the present volumes are not I hope wholly unworthy of the patronage of one who is the Leader and Protector of British Soldiers.

That you may long enjoy these high distinctions, is the sincere wish of

Your Royal Highness's,

Most faithful

And obliged Servant,

JAMES CURRIE.

Bath, July 7, 1805.

TO THE
RIGHT HON. SIR JOSEPH BANKS,

*Baronet, and Knight of the Bath, President of
the Royal Society, &c. &c. &c.*

Liverpool, 31st October, 1797.

SIR,

IN presenting this volume to you,* I beg leave to say a few words on the views with which it was written, and the circumstances that gave rise to the publication.

About eighteen years ago, when I was at Edinburgh, it fell to my lot to write a paper on the influence of cold on the living body, for one of the societies of students, of which I was a member. In defending my speculations against some ingenious opponents, a perpetual contradiction occurred as to facts, which a reference to original authorities, did not enable me to remove; for I discovered, that the accounts given of the temperature of the human body under disease, even by the most approved authors, are, with a few exceptions, founded, not on any exact measurement of heat, but on the sensations of the patient himself or his attendants.

* This Letter was prefixed to the first Edition.

Impressed with the belief, that till more accurate information should be obtained respecting the actual temperature in different circumstances of health, and disease, no permanent theory of vital motion could be established, nor any certain progress made in the treatment of those diseases in which the temperature is diminished or increased, I have occasionally since that time, observed and recorded such facts as related to the subject; intending, one day or other, to lay my observations before the public, if they acquired an importance that deserved attention. In the outset of this undertaking, nothing seemed wanting but accurate thermometers, and a moderate portion of time and attention; and I embraced in imagination the whole effects of temperature upon health and disease; a range of inquiry which experience has convinced me it would be temerity and folly to hope to go through, In this general view of the subject, some valuable communications have however been made to me by my much respected friend Dr. Percival; which do not apply to the particulars treated of in the following volumes, but which I am not without the hopes of specifying at some future period, when I may have occasion to employ them.

Though I have some time seen, that the delay
of

of publishing till my original plan was executed, was likely to render my labours wholly abortive, or to convert the imperfect product from a gift into a legacy; yet I should not have given to the world so detached and unfinished a work as the present, according to the views I had originally entertained, but for the circumstances I am about to relate.

By the accounts received at the beginning of the present year, the fever of the West Indies appeared to continue its desolating progress with little abatement, and in America to be beginning its ravages anew.—With this pestilence, science seemed hitherto to have contended, in a great measure, in vain, and new methods of opposing it, were not merely justifiable, but requisite. At this time an account of the success of the nitric acid in Lues Venerea and Hepatitis, as employed by your correspondent Mr. Scott, of Bengal, was through your means, given to the public. His theory, suggested by the new chemistry, did not appear promising; but it was neither wise nor candid to reject his experience on that account. The first trials which I made of his practice persuaded me, that, though the success of the nitric acid might have been exaggerated by a warm imagination and a benevolent heart, it assuredly did succeed in certain cases of the dis-

cases in question, and that a remedy of *considerable power, and of perfect safety*, was introduced into medicine. These opinions subsequent experience has served to confirm. In the fever of the West Indies mercury had been much employed, ~~and~~ though different notions were entertained of its mode of operation, it seemed on the whole the most approved remedy. Since the nitric acid appeared to be a substitute for mercury in other cases, it seemed reasonable to try its effects in this fatal fever. I suggested this practice by letter to some practitioners in the West Indies, and I submitted my notions on the subject to you; sensible that your character and station might bring that into immediate notice, which the influence of a private individual could but slowly effect.—Your conduct even exceeded my expectation.

Our correspondence on this occasion turned my views to the other means of opposing this pestilence. Ablution with cold water in fever had been so long employed at the hospital here, and in private practice, by my friends and colleagues Dr. Brandreth and Dr. Gerard, as well as myself, that it was become general in Liverpool, and common in the county of Lancaster. So long ago as the year 1791, a general statement by Dr. Brandreth of its advantages had been published

lished by Dr. Duncan, in the Medical Commentaries of that year. It had also been noticed by me in the Philosophical Transactions for 1792, and I had repeatedly mentioned it in private correspondence; it had often been recommended to the surgeons of African ships in those examinations required by the legislature, and which are chiefly made by the physicians and surgeons of our hospital. On different occasions likewise I had not only explained, but exhibited the practice, to practitioners from a distance, and particularly to one or two going to the West Indies. A method of treatment so bold, and so contrary to common prejudices, made however, as it appears, slow progress. The mode of operation of our remedy has been misapprehended; the proper period for using it has not been understood; and on some occasions having been resorted to improperly, the consequences have brought it into disrepute. Reflecting on these circumstances, and exposed by situation to the reiterated sounds of death from the Western World, my decision was speedily made: I resolved no longer to delay an account of our treatment of fever, in the expectation of including it in a larger field of discussion, and of presenting it in a form more conducive to reputation; and the fruit of this determination is the work now presented to you. In treating my subjects, perspicuity has been studied rather

rather than rigorous method ; I have every where endeavoured to make my steps so plain, that they may be distinctly traced ; the most important points are impressed again and again to guard against mistake ; my thermometrical observations have enabled me to give a precision to the directions, for the use of the affusion of cold water, which otherwise they could not have had ; and, if I do not flatter myself, have laid a foundation for my reasonings, which speculations on fever have seldom possessed. I have guarded against the unnecessary use of technical as well as of general expressions. It were better perhaps that medicine, like other branches of natural knowledge, were brought from its hiding-place, and exhibited in the simplicity of science, and the nakedness of truth. If it had been in my choice, I would not have adopted the language of theory, like Boerhaave, or Sydenham ; but have exhibited a medical work in the phraseology that Bacon, had he lived in our days, might have used. Unfortunately in the present state of medical knowledge, wholly to avoid the language of theory is impossible. The corruptions of false doctrines must remain more or less, in our phraseology, after the doctrines themselves are exploded ; since custom has rendered the expressions in which they are found, intelligible, and human sagacity has not yet discovered those first principles of
living

living motion, by which the doctrines and the language of physiology might at once be reformed. Hence the term *re-action* is applied to certain motions of life, though in a sense very different from that in which it is used in the science of inanimate motion from which it is borrowed; and such words as *tone* will still be found in the following pages, though the theory that introduced them into medicine be universally abandoned. The use of such expressions is however an evil, justified only by necessity; and I have endeavoured to avoid it as much as lay in my power.

Possibly this notice may procure me some readers among men of general science; and this I confess to be one of my objects in dedicating the work to you. It is naturally an author's wish that his book may be read by those who can appreciate it, and who from their situations may have it in their power to bring its precepts into practice. In both these points of view I appeal to you—to your scientific knowledge, and to your generous heart. The work that I address to you is in a great measure practical. A man of genius, at the head of a fleet or army, would probably find little difficulty in understanding it; and possibly, if he understood it there might be occasions on which it would afford scope to his humanity and patriotism. But whatever be its fate

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with

with men professedly military, I trust it will not be overlooked by the medical practitioners of our fleets and armies; a most meritorious class of the profession, to whom a great part of the improvements in the modern practice of medicine is to be ascribed.

Conceiving that the circulation of this volume, as well as its usefulness might be extended by connecting the history of the affusion of cold water in fever, with other views of the same remedy, and with a few observations on the other remedies in fever, I have entered on these points, without any very strict regard to method; and have been insensibly led to speak of some of the operations of temperature on the body in health, a subject which I had reserved. Such as it is, this volume may serve as the first of a series on similar subjects, if I should ever write them; and it may, I hope, stand alone, if I should write no more.

I am sensible that some of these particulars would have appeared with more propriety in a professed preface: but having entered on certain explanations in my address to you, I have given the whole of these preliminary observations in the same form; a freedom that I trust you will forgive.

I cannot conclude without declaring the sense I entertain of your candour and politeness. Accept the tribute of my respect.—May you live long to cultivate and protect the sciences—the sciences, whose utility is beyond dispute; whose progress is superior to obstruction; and which, of all the possessions of man, seem least to partake of the imperfection of his nature !

I have the honour to be,

Sir,

Your faithful

And very obedient Servant,

JAMES CURRIE.

Advertisement to the third Edition.

AN Apology is perhaps required for the delay which has occurred in presenting the third edition of this work to the Public, and especially to those Gentlemen by whose communications it is enriched.

The immediate pressure of my professional duties, and an imperfect state of health, have occasioned this delay. These circumstances must also plead my excuse for the work being printed with less accuracy than could be wished, and without the advantage of a new arrangement, which it was once my intention to have given it. But the copious Table of Contents and Index, will in some measure supply this defect.

In the Table of Contents, the additions made to this Volume, since its first publication, are in general marked with an asterisk.

J. C.

May, 1804.

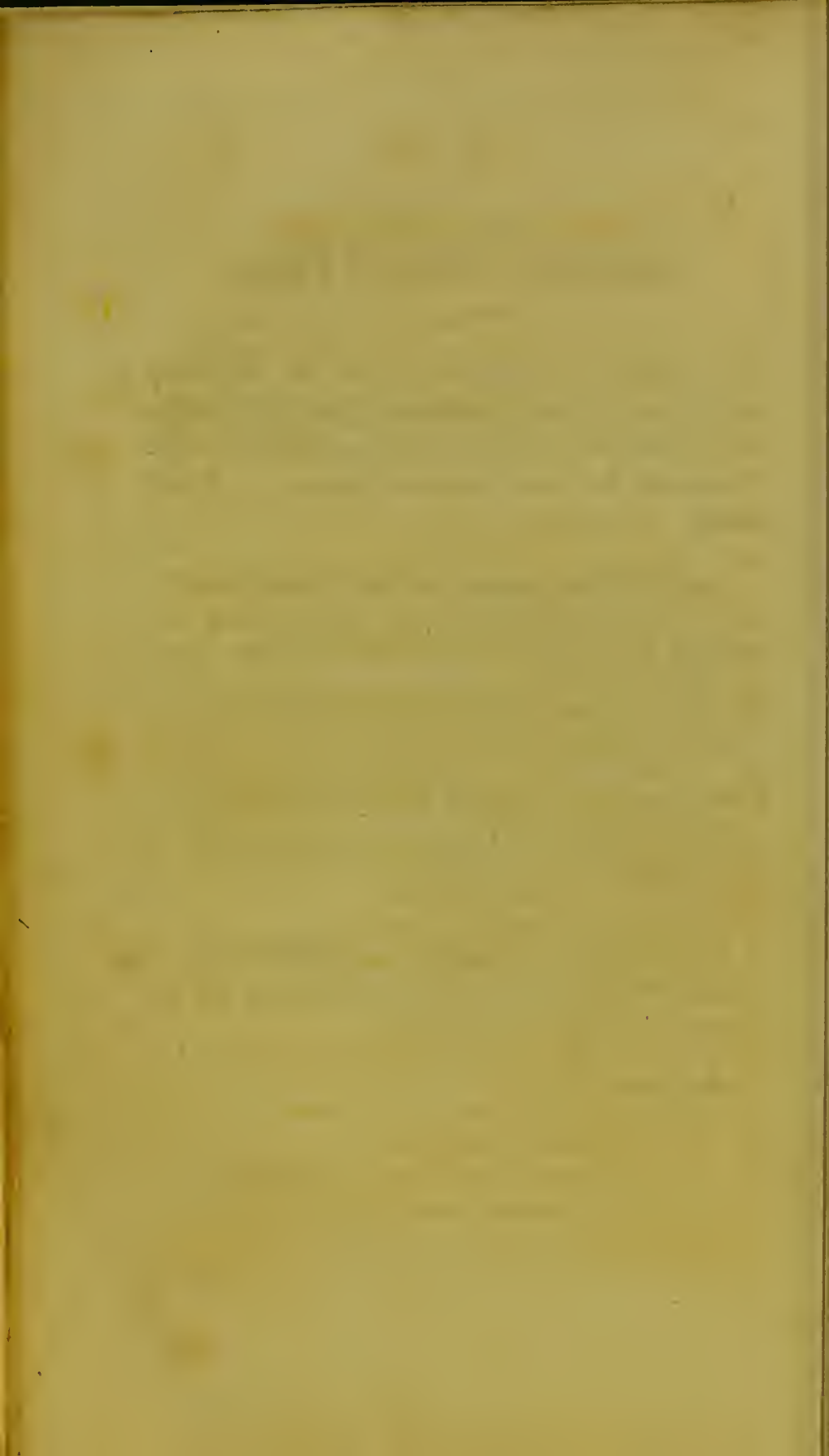


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MEDICAL REPORTS, &c.

CHAP. I.

Narrative of Dr. Wright.

IN a number of the London Medical Journal, published in the Summer of the year 1786, Dr. William Wright*, formerly of the island of Jamaica, gave an account of the successful treatment of some cases of fever by the ablution of the patient with cold water.

“ On the 1st of August, 1777,” says Dr. Wright, “ I embarked in a ship bound to Liverpool, and sailed the same evening from Montego Bay. The master told me he had hired several sailors on the same day we took our departure ;

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* Now President of the College of Physicians, Edinburgh, (1803.)

one of whom had been at sick quarters on shore, and was now but in a convalescent state. On the 23d of August we were in the latitude of Bermudas, and had had a very heavy gale of wind for three days, when the above-mentioned man relapsed, and had a fever, with symptoms of the greatest malignity. I attended this person often, but could not prevail with him to be removed from a dark and confined situation, to a more airy and convenient part of the ship; and as he refused medicines, and even food, he died on the eighth day of his illness.

“ By my attention to the sick man I caught the contagion, and began to be indisposed on the 5th of September, and the following is a narrative of my case, extracted from notes daily marked down: I had been many years in Jamaica, but, except being somewhat relaxed by the climate, and fatigue of business, I ailed nothing when I embarked. This circumstance, however, might perhaps dispose me more readily to receive the infection.

“ Sept. 5th, 6th, 7th, Small rigors now and then—a preternatural heat of the skin—a dull pain in the forehead—the pulse small and quick—a loss of appetite, but no sickness at the stomach—the tongue white and slimy—little or no thirst—the
 5 belly

belly regular—the urine pale and rather scanty—in the night restless, with starting and delirium.

“ Sept. 8th, Every symptom aggravated, with pains in the loins and lower limbs, and stiffness in the thighs and hams.

“ I took a gentle vomit on the second day of this illness, and next morning a decoction of tamarinds; at bed-time, an opiate, joined with antimonial wine; but this did not procure sleep, or open the pores of the skin. No inflammatory symptoms being present, a drachm of Peruvian bark was taken every hour for six hours successively, and now and then a glass of port wine, but with no apparent benefit. When upon deck, my pains were greatly mitigated, and the colder the air the better. This circumstance and the failure of every means I had tried, encouraged me to put in practice on myself what I had often wished to try on others, in fevers similar to my own.

“ Sept. 9th, Having given the necessary directions, about three o'clock in the afternoon I stripped off all my cloaths, and threw a sea-cloak loosely about me till I got upon the deck, when the cloak also was laid aside: three buckets full of salt water were then thrown at once upon me; the shock was great, but I felt immediate relief.

The head-ach and other pains instantly abated, and a fine glow and diaphoresis succeeded. Towards evening, however, the same febrile symptoms threatened a return, and I had again recourse to the same method as before, with the same good effect. I now took food with an appetite, and for the first time had a sound night's rest.

“ Sept. 10th, No fever, but a little uneasiness in the hams and thighs—used the cold bath twice.

“ Sept. 11th, Every symptom vanished, but to prevent a relapse, I used the cold bath twice.

“ Mr. Thomas Kirk, a young gentleman, passenger in the same ship, fell sick of a fever on the 9th of August. His symptoms were nearly similar to mine, and having taken some medicines without experiencing relief, he was desirous of trying the cold bath, which, with my approbation, he did on the 11th and 12th of September, and, by this method, was happily restored to health. He lives at this time (Jan. 1786) near Liverpool.”

To this interesting narrative, Dr. Wright adds some general observations on the traces that are to be found of the use of cold water internally and externally in fevers, in several works ancient and modern. But whether he himself pursued this practice any farther, I have not been informed.

Having

Having before experienced that Dr. Wright was a safe guide*, I immediately on reading this narrative, determined on following his practice in the present instance; and before an opportunity occurred of carrying my intention into effect, I was farther encouraged, by learning, that my respectable colleague, Dr. Brandreth, had employed cold water externally in some recent cases of fever with happy effects.

* See a paper in the Memoirs of the London Medical Society, vol. iii. p. 147, to be found in a subsequent part of this volume.

CHAP. II.

History of a fever which broke out in the Liverpool Infirmary.

ON the 9th of Dec. 1786 *, a contagious fever made its appearance in the Liverpool Infirmary. For some time previously the weather had been extremely cold, and the discipline of the house, owing to causes which it is unnecessary to mention, had been much relaxed. The intensity of the cold prevented the necessary degree of ventilation, and the regulations for the preservation of cleanliness had been in some measure neglected. These circumstances operated particularly on one of the wards of the eastern wing, employed as a lock-hospital for females, where the contagion first appeared. The fever spread rapidly, and before its progress could be arrested, sixteen persons were affected, of which two died. Of these sixteen, eight

* The year was marked in the former editions, 1787, by mistake, as may be seen by a reference to the books of the hospital.

were under my care. On this occasion I used for the first time the affusion of cold water, in the manner described by Dr. Wright. It was first tried in two cases only, the one on the second, the other on the fourth day of fever. The effects corresponded exactly with those mentioned by him to have occurred in his own case; and thus encouraged, I employed the remedy in five other cases. It was repeated daily, and of these seven patients, the whole recovered.—In the eighth case, the affusion of cold water seemed too hazardous a practice, and it was not employed. The strength of this patient was much impaired by lues venerea, and at the time of catching the contagion, she laboured under ptyalism. I was not then aware that this last circumstance formed no objection against the cold affusion, and in a situation so critical, it was thought imprudent to use it. The usual remedies were directed for this patient, particularly bark, wine, and opium, but unsuccessfully; she died on the 16th day of her disease.

From this time forth, I have constantly wished to employ the affusion of cold water in every case of the low contagious fever, in which the strength was not already much exhausted; and I have preserved a register of a hundred and fifty-three cases, in which the cure was chiefly trusted to this remedy.

dy. Of these, ninety-four occurred in the hospital in the four years subsequent to the period already mentioned, twenty-seven in private practice, and thirty-two in the 30th regiment of foot, when quartered in Liverpool in the year 1792. Of late (1797) I have not thought it necessary to register all the cases in which this remedy has been employed. Having satisfied myself of its extraordinary efficacy, and of the precautions necessary in using it, I have found it the shorter method, as well as the more instructive, to record the instances in which it has proved unsuccessful. To detail the whole of my experience would be a tedious and an useless labour. I purpose to digest the results under a few distinct heads, supporting and illustrating each general proposition by an ample detail of cases. Before, however, this preliminary account is closed, it will be useful to enter more particularly into the history of the contagious fever which broke out in the 30th regiment, because the account of its rise, progress, treatment, and termination, will support in a striking manner, the doctrines I wish to establish, and if I do not greatly deceive myself, may afford important instruction, as well as encouragement, to those whose duty may call them to oppose the progress of contagious fever in similar situations.

CHAP. III.

*History of a fever which occurred in the 30th
regiment.*

THE 30th regiment, as is usual with troops in Liverpool, was billeted in the town, but paraded and mounted guard in the fort, situated north of the town, on the banks of the river. The general guard-room had been used previous to the arrival of the 30th, as a place of confinement for deserters; it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. This water was now half evaporated, and from the surface issued offensive exhalations.

In a dark, narrow, and unventilated cell, off the guard-room, it was usual to confine such men as were sent to the guard for misbehaviour, and about the 20th of May, 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours, under the debility that succeeds intoxication. The

typhus, or jail fever, made its appearance in two of these men about the first of June, and spread with great rapidity. Ten of the soldiers labouring under this epidemic, were received into the Liverpool Infirmary, and the wards allotted to fever could admit no more. The contagion continuing its progress, a temporary hospital was fitted up at the fort, and I was requested to give my assistance there to the surgeon of the regiment, by Captains Brereton* and Torriano.†

In two low rooms, each about fifteen feet square, were fourteen patients labouring under fever. They were in different stages of its progress: one was in the fourteenth day of the disease, two were in the twelfth, and the rest from the ninth to the fourth inclusive. The symptoms of the fever were very uniform. In every case there was more or less cough, with mucous expectoration: in all those who had sustained the disease eight days and upwards, there were petechæ on the skin; in several there were occasional bleedings from the nostrils, and streaks of blood in the expectoration. The debility was considerable from the

* Now (1798) Colonel Brereton of the 63d foot.

† The gallant and accomplished officer who fell soon after at the heights of Pharon, in the defence of Toulon.

the first, and it had been increased in several cases by the use of venesection, before the nature of the epidemic was understood. The pulse varied from 130 strokes in the minute to 100; the heat rose in one case to 105° of Fahrenheit, but was in general from 101° to 103° ; and towards the latter stages of the disease it was scarcely above the temperature of health.—Great pain in the head, with stupor, pervaded the whole, and in several instances there occurred a considerable degree of the low delirium.

Our first care was to ventilate and clean the rooms, which were in a high degree foul and pestilential. Our second was to wash and clean the patients themselves. This was done by pouring sea-water, in the manner already described, over the naked bodies of those whose strength was not greatly reduced, and *whose heat was steadily above the temperature of health*. In those advanced in the fever, whose debility was of course great, we did not venture on this treatment, but contented ourselves with spunging the whole surface of the body with tepid vinegar, a practice, that in every stage of fever is salutary and refreshing.

Our next care was to stop the progress of the infection. With this view, the guard-house was at first attempted to be purified by washing and
ventilation,

ventilation, the greatest part of its furniture having been burnt, or thrown into the sea. All our precautions and exertions of this kind were however found to be ineffectual. The weather was at this time wet, and extremely cold for the season; the men on guard could not be prevailed on to remain in the open air: and from passing the night in the infected guard-room, several of the privates of the successive reliefs, caught the infection, and fell ill on the 10th, 11th, and 12th of the month. In several of these the fever ran through its course; and in others, it was immediately arrested by the affusion of sea-water as already described. No means having been found effectual for the purification of the guard-room, it was shut up, and a temporary shed erected in its stead. Still the contagion proceeded; on the morning of the 13th three more having been added to the list of the infected. On that day, therefore, the whole regiment was drawn up at my request, and the men examined in their ranks: Seventeen were found with symptoms of fever upon them.—It was not difficult to distinguish them as they stood by their fellows. Their countenances were languid, their whole appearance dejected, and the tunica adnata of their eyes had a dull red suffusion. These men were carefully separated from the rest of the corps, and immediately subjected to the cold affusion, always repeated once, and sometimes

times twice a day.—In fifteen of the number the contagion was extinguished ; but two went through the regular disease. On the same day, the commanding officer, at my desire, issued an order for the whole of the remaining part of the regiment to bathe in the sea ; and for some time they were regularly mustered, and marched down at high water, to plunge into the tide.

These means were successful in arresting the epidemic : after the 13th of June no person was attacked by it. It extended to fifty-eight persons in all, of which thirty-two went through the regular course of the fever, and in twenty-six the disease seemed to be cut short by the cold affusion. Of thirty-two already mentioned, two died. Both of these were men whose constitutions were weakened by the climate of the West Indies ; both of them had been bled in the early stages of the fever ; and one of them being in the twelfth, the other in the fourteenth day of the disease when I first visited them, neither of them was subjected to the cold affusion. The water employed on this occasion was taken up from the river Mersey close by the fort. It was at that time of a temperature from 58° to 60° of Fahrenheit, and it contains in solution from a 32d to a 33d part of sea-salt.

In hospitals, manufactories, and prisons—situations

tions in which the low contagious fever so frequently originates, the practice I have detailed may be followed with great ease, safety, and advantage; but it is in a more particular degree applicable to this contagion when it appears on ship-board, because in that situation the usual means of prevention or cure are necessarily limited, and the imminence of the danger requires a remedy that operates with speed as well as efficacy. The waters of the ocean afford this remedy; in every point of view a most happy one for mariners, since it can be applied almost as easily as it can be procured.

CHAP. IV.

The manner in which the affusion of cold water ought to be used in fever.

HAVING given this general account of my experience of this remedy in fever, it will now be necessary to enter more particularly on the rules which ought to govern its application, and on the different effects to be expected from it, according to the different stages of the disease in which it is employed. It will be proper to premise, that when the term fever is used in the present work without any adjuncture, it is the low contagious fever that is meant. This is the Typhus of Dr. Cullen; the contagious fever of Dr. Lind; the Febris inirritativa of Dr. Darwin. In popular language, it is generally called the nervous fever, and where particular symptoms appear, the putrid fever. It is usually produced in situations where there is a want of cleanliness, and more especially of ventilation: and when produced, it is propagated by contagion. This is the common fever of England;

land; its symptoms have been detailed with great minuteness in a variety of modern publications, and I have therefore declined repeating descriptions that are every where to be met with. Dr. Cullen has defined the disease as follows:—"Morbus
 "contagiosus; calor parum auctus; pulsus parvus,
 "debilis, plerumque frequens; urina parum mutata; sensorii functiones plurimum turbatae;
 "vires multum imminutæ." In sixteen years practice I have found the contagious fever of Liverpool remarkably uniform, and in general to correspond exactly with this concise and perspicuous definition. This disease prevails chiefly among the poor, who from the nature of their diet and habits, are peculiarly exposed to the causes that produce it. Seldom extending itself in any considerable degree among the other classes of the community, it has been supposed that Liverpool was little subject to fever; but this will be shown from authentic documents, to be a great and a pernicious error. Let us proceed at present to inquire into the rules that ought to govern the use of the affusion or aspersion of cold water in this disease.

Whoever has watched the progress of fever, must have observed the justness of the observation made by Cullen, Vogel, De Haen, and others, that even those genera which are denominated continued,

tinued, are not strictly such, but have pretty regular and distinct exacerbations and remissions in each diurnal period. In this space of time, Dr. Cullen contends, that an attentive observer may commonly distinguish two separate paroxysms.* My observations do not enable me to confirm his position in its full extent—but one exacerbation, and one remission in the twenty-four hours, seem generally observable. The exacerbation usually occurs in the afternoon or evening, the remission towards morning. These exacerbations are marked by increased flushing, thirst, and restlessness. If the heat of the patient be, at such times, taken by the thermometer, it will be found to have risen one or two degrees in the central parts of the body above the average heat of the fever, and still more on the extremities.—The safest and most advantageous time for using the aspersion or affusion of cold water, is when the exacerbation is at its height, or immediately after its declination is begun; and this has led me almost always to direct it to be employed from six to nine in the evening; but it may be safely used at any time of the day, *when there is no sense of chilliness present, when the heat of the surface is steadily above what*

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* Paroxysmis quovis die binis.

is natural, and when there is no general or profuse sensible perspiration.—These particulars are of the utmost importance.

1. If the affusion of cold water on the surface of the body be used during the cold stage of the paroxysm of fever, the respiration is nearly suspended; the pulse becomes fluttering, feeble, and of an incalculable frequency; the surface and extremities become doubly cold and shrivelled, and the patient seems to struggle with the pangs of instant dissolution. I have no doubt, from what I have observed, that in such circumstances, the repeated affusion of a few buckets of cold water would extinguish life. This remedy should therefore never be used when any considerable sense of chilliness is present, even though the thermometer, applied to the trunk of the body, should indicate a degree of heat greater than usual.

2. Neither ought it to be used, when the heat, measured by the thermometer, is less than, or even only equal to the natural heat, though the patient should feel no degree of chilliness. This is sometimes the case towards the last stages of fever, when the powers of life are too weak to sustain so powerful a stimulus.

3. It is also necessary to abstain from the use

of this remedy when the body is under profuse sensible perspiration, and this caution is more important in proportion to the continuance of this perspiration. In the commencement of sweating, especially if it has been brought on by violent exercise, the affusion of cold water on the naked body, or even immersion in the cold bath, may be hazarded with little risque, and sometimes may be resorted to with great benefit. After the sweating has continued some time and flowed freely, especially if the body has remained at rest, either the affusion or immersion is attended with danger, even though the heat of the body at the moment of using it be greater than natural.—Sweating is always a cooling process in itself, but in bed it is often prolonged by artificial means, and the body is prevented from cooling under it to the natural degree, by the load of heated clothes. When the heat has been thus artificially kept up, a practitioner, judging by the information of his thermometer only, may be led into error. In this situation, however, I have observed that the heat sinks rapidly on the exposure of the surface of the body even to the external air, and that the application of cold water, either by affusion or immersion, is accompanied by a loss of heat and a deficiency of re-action, which are altogether inconsistent with safety.—Each of these points will be illustrated more fully in the sequel.

Under these restrictions the cold affusion may be used at any period of fever; but its effects will be more salutary in proportion as it is used more early. When employed in the advanced stages of fever, where the heat is reduced and the debility great, some cordial should be given immediately after it, and the best is warm wine. The general effects of the cold affusion will be more clearly illustrated by the following cases. They are a selection from a great number, the records of which have been preserved, and which lead to the same results. They are so arranged as to exhibit the salutary effects of this remedy in the different stages of fever, and illustrate the precautions laid down against using it improperly. If they should appear tedious after what has already been mentioned, this must be forgiven; on a subject so important and so little understood, it is better to incur the charge of tediousness than the hazard of being obscure.

1804. The presence of severe diarrhœa, or dysentery, seems to forbid the use of the cold affusion, or at least to render its advantage uncertain.—See Vol. II.

CHAP. V.

CASE I.

Cases in which the Affusion of cold water was used in the different stages of fever.

January 1, 1790.

A Nurse in the fever-ward of the Infirmary having several patients under her care, caught the infection. She was seized with violent rigors, chilliness and wandering pains, succeeded by great heat, thirst, and head-ach. Sixteen hours after the first attack, her heat at the axilla was 103° of Fah^t. her pulse 112 in the minute and strong: her thirst great, her tongue furred, and her skin dry.

Five gallons of salt water, of the temperature of 44° were poured over her naked body, at five o'clock in the afternoon, and after being hastily dried with towels, she was replaced in bed; when
the

the agitation and sobbing had subsided, her pulse was found to beat at the rate of 96 strokes in the minute, and in half an hour afterwards it had fallen to 80. The heat was reduced to 98° by the affusion, and half an hour afterwards it remained stationary. The sense of heat and head-ach were gone, and the thirst nearly gone. Six hours afterwards she was found perfectly free of fever, but a good deal of debility remained.

Small doses of colombo were ordered for her, with a light nourishing diet, and for several days the cold affusion was repeated at the same hour of the day as at first; the fever never returned.

During the progress of fever when epidemic, a great number of cases similar to the above have occurred, in which the disease was suddenly cut short by the use of the cold affusion on the first and second day; twenty-six of these cases were in the 30th regiment, as has been already stated. In such instances, the result was so precisely similar to what occurred in the case I have related, that it would be to no purpose to detail them.

When an epidemic fever is spreading, and the danger is known, patients will take the alarm on the

the first attack, and the power, as well as the utility of such a remedy as the cold affusion, in such situations of general danger, will be easily imagined.—It cannot be employed too soon after the first attack, provided the original chill is over, and the hot stage is firmly established.

In cases in which the affusion was not employed till the third day of fever, I have seen several instances of the same complete solution of the disease. I have even seen this take place when the remedy had been deferred till the fourth day; but this is not common.—The following case will point out the usual effects of this remedy in the third and fourth days of the disease.

CASE II.

Jan. 17, 1790. A. B. aged nineteen, a pupil of the Infirmary, caught the infection in attending the fever-ward. When I saw him, seventy-eight hours had elapsed since the first attack; he was of course in the fourth day of the disease. He had all the usual symptoms—head-ach, thirst, furred tongue, pain in the back and loins, with great debility. His heat was 101° , and his pulse 112 in the minute.

A bucket full of salt water was poured over him as usual, at noon on the 17th. His heat sunk to 99° , and his pulse to 98 in the minute. A profuse perspiration followed, with the cessation of all his feverish symptoms.—This intermission continued for several hours, during which he enjoyed some comfortable sleep: but at five in the afternoon he was again seized with feverish rigors, followed by heat, thirst, and head-ach as before. An hour afterwards, the hot stage was established; his heat was 100° , his pulse 100. The same quantity of cold water was again poured over him, and with similar effects. His pulse fell immediately to 80, and became more full: his heat became natural. The following night he took twenty drops of laudanum and slept well.

On the 18th at noon his pulse was 96 and soft; his skin moist, but a little above the natural heat. His tongue was a little furred, and his head ached: he also complained of thirst; the heat at the axilla was 100° .—The same remedy was again applied. He was greatly refreshed by it. The pulse fell to 90, the skin became cool, the thirst went off, and all the feverish symptoms vanished.

On the 19th his pulse was 88, his heat natural, the thirst and head-ach were gone, and his
appetite

appetite improving. The affusion was repeated for the last time at six o'clock in the evening.

On the 20th his pulse was 78 and soft, his tongue clean, and his appetite farther improved. He had still some remains of debility on the 21st, but on the 22d he was free of complaint. This patient during his fever took no medicine but the effervescing mixture, the dose of laudanum excepted—The affusion was used four times.

CASE III.

Dec. 8, 1791. A woman aged fifty-seven, who had caught the infection in her attendance on a poor family labouring under fever, came under my care in the hospital, fifty hours after the first attack. She was of course in the third day of fever; she had the usual symptoms—head-ach, pain in the back and loins, and thirst: her tongue was furred and her eyes heavy: her pulse 96, her heat 101° . The affusion of cold water was performed at noon. In a few minutes afterwards the heat under the tongue was 98° , the pulse 80. Towards evening however, the feverish rigor returned with all the usual consequences. As soon as the hot stage was established; the affusion was repeated, and with the usual happy effects.

Dec.

Dec. 9.—Noon—pulse 90 and feeble—respiration easy—heat 100° .—The affusion was immediately employed, and again repeated in the evening, sensible perspiration, coolness, and quiet sleep, were the consequence.

Dec. 10. This day the affusion was twice repeated as yesterday, viz. at noon, and at six in the evening.—At eight in the evening the pulse was 76, the skin soft, the heat 97° , the respiration easy and natural.—The fever returned no more. This patient used no other remedies but an enema, and after it an opiate, every night. The affusion was used twice every day; in all six times.

CASE IV.

Feb. 2. 1792. S. C. a healthy man, aged forty-four, about seventy-two hours after the first attack of fever, became my patient. His pulse 100—his heat 104° .—The other symptoms as usual—but the pain in the head and back particularly severe. Two minutes after the affusion—pulse 90, heat 101° ; six minutes after—pulse 90 and weak;—ten minutes after, pulse 90, heat 100° .—This patient felt great refreshment, and was entirely relieved of the pain in the head and back.—In the evening however the exacerbation of fever was severe, and the

the head-ach returned with violence.—He passed a restless night. About four o'clock in the ensuing morning, the affusion was repeated by his own desire. At 9, A. M. a general and gentle perspiration covered the surface of the body; the pulse was 84, the tongue moist, the skin cool, and the pains of the head and back entirely gone off. In the afternoon, however, the fever returned, though in a slighter degree. The affusion was repeated the fourth time with the same happy effects, and after this he had no return of the disease.

Thus it appears, that the cold affusion used on the third and fourth days of fever, does not usually produce an immediate solution of the disease; but that it instantly abates it, and by a few repetitions, brings it to a happy termination in two or three days.

CASE V.

Oct. 25. M. S. came under my care on the 24th Dec. 1791, on the seventh day of typhus, with the usual symptoms—pulse 108, heat 100°.—The cold affusion was immediately directed—two minutes afterwards the heat was 96°—three minutes afterwards 98°—the pulse 98. This patient experienced

experienced great relief. The affusion was repeated on this and the following day—a gentle diaphoresis always succeeded it with tranquil sleep, and on the third day the fever was completely removed.

CASE VI.

A young lady of 19, in the 7th or 8th day of typhus, became my patient, Sept. 26, 1794. Her pulse was 112 and feeble, heat 101° . She had great pain in the head, and much prostration of strength, her eyes were suffused and dull—her tongue furred—her spirits greatly depressed. Saline medicines were used for this patient, with lemonade for her usual drink, and moderate quantities of wine were given mixed with water. The burning sensation in the palms of her hands and temples was assuaged by frequent spunging with vinegar, and every evening at six P. M. three or four gallons of cold brine were thrown over her. The happy effects so frequently described, were in this case particularly striking:—The pulse fell almost immediately to 90, the heat to its natural standard, and the head-ach vanished—a gentle diaphoresis followed, with easy sleep:—in a few hours, however, the feverish symptoms returned, and towards the hour of six in the evening, the fever was in its highest state of exacerbation. At this hour there-
fore

fore the affusion was repeated with the same happy effects—though the fever returned as before, it was in a milder form; the same practice was continued, and on the second of October, she was entirely free from the disease.

CASE VII.

F. G. a soldier of the 30th regiment, aged 33, fell under my care on the 9th of June, 1792, during the prevalence of the epidemic in that regiment, of which I have already given an account. He was in the 9th day of the disease—his pulse 100 and feeble—his heat 104° —his thirst was very great—his tongue foul and black—frequent cough occurred, with streaks of blood in the expectoration—and petechiæ appeared all over his body. His mind was at all times confused, and at times he was completely delirious. I directed that his strength should be supported by administering a bottle of wine every day, with an equal quantity of gruel;—that every night he should take an opiate draught, and that a complete operation of his bowels should be procured by a clyster administered daily, and if this did not succeed, by a few grains of calomel. I also directed that a bucket full of salt-water should be thrown over him immediately, and repeated according to circumstances. In a few minutes after the affusion,

the heat was 98° —the pulse 98—his mind was more calm and collected: two hours afterwards he had relapsed into nearly his former state, but the night was passed more tranquilly. The whole of this practice was continued with nearly the same result, till the 12th day of the disease, the affusion having always been performed in the evening, and sometimes at noon also. The fever continued its usual period, but on the 12th day, the heat having sunk to its natural standard, the cold affusion was thenceforth omitted; we, however, spunged the whole body once or twice a day with vinegar.—The patient was in a state of convalescence on the 18th day from the first attack.

I have related this case the more circumstantially, because it contains the particulars of my practice in the epidemic in which it occurred, at the same time that it affords an example of the effects of the cold affusion used in the more advanced periods of fever. In such instances, as might be expected, it does not procure the same advantages as in the earlier stages, when the strength is less impaired, and the morbid actions less firmly associated; nevertheless it is evidently advantageous while the heat of the patient exceeds the natural standard, though it ought to be employed with caution in the more advanced stages

of the disease, and in such cases, according to my later experience, of a temperature from fifteen to twenty degrees only, below the human heat. In the greater part of the cases that I have related, the water employed was the pump-water of our hospital, saturated with sea-salt, and of a temperature from 40° to 50° of Fah°.

The cold affusion may also be applied with success in intermittent fevers, as I have found by repeated trials, and as the following case will demonstrate.

CASE VIII.

Ann Hall, aged 22, was admitted into the Infirmary, July 19, 1792, under an obstinate quotidian of three months standing—she had from time to time taken the bark, but as the great delicacy of her stomach, would not permit her to use it in sufficient quantities, she was become very feeble and much emaciated. A gentle emetic was administered to her in the first instance, and on the commencement of the hot stage of the paroxysm, twenty drops of the tincture of opium were directed to be given to her, after the practice of Dr. Lind. During the intermission, the bark was ordered to be taken in such doses as her stomach would bear; she was put on a nourishing diet, and

was

was ordered a pint of port wine every day. This plan was pursued for fourteen days, but without success; the paroxysms returned daily, though with some irregularity—her strength was however rather improved. Still the delicacy of her stomach continued, and the bark, except in very small doses, was constantly rejected.

On the 8th of August, two hours before the expected accession of the fever, four gallons of brine were dashed over her, of the temperature of 66° of Fah^t., and this day she escaped the attack. In the interval between this and the period of the next return, she took the bark in larger quantity, the power of her stomach being increased; but on the 10th, two hours later than usual, the paroxysm returned with unusual severity. *After the hot stage was completely formed*, the brine was poured over her, as before—the symptoms instantly abated; she fell into a gentle perspiration, with profound sleep. She afterwards continued the bark as before, and from this time forward was free of disease.

It would be easy to multiply these details, but their uniformity has already perhaps rendered them tedious; a few general observations shall therefore conclude this division of the subject.

CHAP. VI.

General Observations.

1. **THOUGH** the patients were often startled at the first proposal of dashing the cold water over them, yet, after one trial, there was seldom any difficulty in persuading them to have it repeated. The effects were in general highly grateful and refreshing to their sensations; the extinction or abatement of fever was commonly followed by more or less diaphoresis, and this again by refreshing sleep.

2. At first I used fresh water—afterwards fresh water mixed with vinegar—and lastly, a saturated solution of sea-salt in water. In the instance of the 30th regiment, I used the water of the river, which contains about a thirty-third part of salt, as has been already mentioned, and this I commonly
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employ

employ in private practice * I was led to prefer salt water to fresh on account of the stimulating effect of sea-salt on the vessels of the skin, by which I apprehend the debilitating action of cold is prevented. Salt water, either for the purpose of immersion or affusion, is more grateful to the patient than fresh water, and it is well known that it may be applied to the surface for a length of time, with much less hazard. Persons immersed in sea water, and especially in saturated brine, for some time together, preserve the lustre of the eye and the ruddiness of the cheek, longer than those in fresh water, of an equal temperature, and such persons exhibit the vital re-action stronger when

* It may be supposed, that the degree of impregnation of the river water with salt must depend on the time of the tide when it is taken up ; thus, that it must be salter at the height of the flood, when the tide has run six hours from the sea, than at the period of low water, when it has run nearly the same time from the land.---I expected to find this the case, and endeavoured to ascertain the difference ; but in a trial which I made by evaporating 40lbs. of water taken up at the height of the flood tide, and the same quantity taken up at low water, I could perceive no difference in the proportion of salt, a circumstance which I am unable to explain. The small difference that there was in the residuum in favour of the water taken up at the top of the tide, arose evidently from its being mingled with a large portion of dirt—the water at the top of the tide is mixed with dirt and feculencies, that taken at the lowest point of ebb is nearly pure.

when removed from it. I preferred the brine to vinegar, as being cheaper, and more easily procured of the necessary quantity: otherwise, it is well known how grateful vinegar is to patients in fever, and perhaps a mixture of vinegar and water of the proper strength, might be preferable even to brine. But though I gave the preference to brine over fresh water, I have very often used the latter, and it is seldom that any danger can result from the want of a saline impregnation, where the cold is employed in so stimulating a form as that which has been described; that is, suddenly, and for so temporary a duration.

3. In taking the heat of the patient, I have generally used a small mercurial thermometer of great sensibility, with a moveable scale, made for me by Mr. Ramsden, after a form invented by the late Mr. Hunter, and used by him in his experiments on the heat of animals, and I have introduced the bulb under the tongue with the lips close, or under the axilla, indifferently; having found by repeated experiments, that the heat in these two places corresponds exactly, and gives a just indication of the heat of the surface of the body, where sheltered by the necessary teguments from the contact of the external air.

Finding, however, considerable risque in using
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the straight tubed thermometer in contagious diseases, I got some instruments of this kind made with a small bulb and curved at the end. The bulb being introduced under the tongue or the axilla, the observer can stand behind the patient, and mark the rise of the mercury, without coming into the immediate sphere of his respiration. Though no injury was in any case incurred from the use of this thermometer, yet a farther improvement has suggested itself. By introducing a small piece of iron into the tube, after the manner of Mr. Six, a permanent indication of the greatest heat is obtained, and the approach of the observer towards the patient during the experiment, is rendered unnecessary.

CHAP. VII.

*Cautions requisite in using the cold affusion,
illustrated by Cases.*

1. IT was before remarked that the cold affusion cannot be used with safety during the cold stage of the febrile paroxysm: the following case will illustrate this truth. In the summer of 1792, I was requested by Mr. Hoffman, an ingenious Prussian gentleman, and a surgeon in the army then under the command of the Duke of Brunswick, to give him an opportunity of seeing the method of using this remedy. At that time there was a patient labouring under a tertian intermittent under my care in the infirmary, on whom it could with propriety be exhibited. Accordingly a time for meeting Mr. Hoffman in the fever-ward was appointed, when the hot stage of the paroxysm might be expected to be fairly formed. It happened however that the accession of the fever had occurred an hour later this day than might have been expected, and
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when we arrived, the patient was still in the cold stage of the paroxysm ; the assistants however proceeded : he was taken out of his bed shivering, his pulse small and frequent, his extremities shrunk and cold. In this state the brine was dashed over him as usual, but not with the usual happy effects ;—his breathing was for some minutes almost suspended ; his pulse at the wrist was not to be felt ; the pulsations of the heart were feeble and fluttering ; a deadly coldness spread over the surface ; and when respiration returned, it was short, irregular, and laborious.—After the use of frictions on the surface, and particularly on the extremities—of a steady warmth applied for some time to the *scrobiculus cordis*—and of cordials cautiously administered in small quantities—the pulse at the wrist returned ; but for some time it was excessively quick and feeble. He recovered however in the course of an hour, and it was found that the paroxysm of fever had been extinguished : but the circumstances first related, were evidently full of danger, and they produced at the time much apprehension and uneasiness. The same remedy was however used in the hot stage of the ensuing paroxysm, and with the usual happy effects.—Other cases to the same purpose might be adduced if it were necessary.

I have frequently used the cold affusion in the
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hot stage of the paroxysm of intermittents, and almost always with the immediate solution of the fit; but in general, if no remedy be used in the intermission, the fever returns at the usual period. In some instances, however, the succeeding paroxysm has been prevented by using the cold affusion about an hour previous to the period of its expected return, and the disease ultimately removed by continuing this practice through four or five of the following periods.

The use of the cold affusion in the absence of fever, requires however a constitution in a great measure unbroken; and many of the intermittents which we see in Liverpool, being transmitted to us from the warm climates, adhere to constitutions in which this practice is not perfectly safe. In such cases, it may notwithstanding be adopted in the hot stage of fever with safety and advantage. Indeed it ought never to be forgotten, that an application of cold, which is safe in the violence of fever, is not safe when the fever is removed. Injury has sometimes occurred from continuing the cold affusion in the period of convalescence.

2. Neither is the cold affusion safe after the sweating stage of fever has continued some time, and the body is passing through that cooling process. The following case will illustrate this position.

tion. In the summer of 1791, a boy of eight years of age, in whom I am peculiarly interested, was attacked by fever. On the third day his pulse rose to 130, and 140 in the minute, and his heat to 106° and 107° of Fah^t. His thirst was very great, and delirium commenced on the second day, and continued without intermission. Various methods had been employed to abate the fever, and particularly to excite sensible perspiration, but unsuccessfully. His heat was not lessened by repeated spunging of the surface of the body with cold vinegar and water; and after a copious bleeding, all the symptoms were as alarming as before. It seemed hazardous to repeat this evacuation, as the blood exhibited no size, and there was a suspicion that the disease originated in contagion. The patient had taken antimonials without any apparent effect, and after watching the state of the thermometer, with the bulb at the axilla, upwards of an hour, though the mercury had sunk a single degree in that interval, it stood at the end of the time as high as 106° . In this state of things we resolved on trying the cold affusion, and every thing being prepared, he was stripped naked and lifted out of bed. As we were about to throw the water upon him, it was observed that a sensible perspiration had broken out all over him, but the heat being so great, we persisted in our purpose, and four gallons of fresh water of the temperature
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of 60°, were dashed upon him; the effects were altogether surprising. On replacing him in bed, the mercury in the thermometer (the bulb at the axilla as before) rose to 98° only, and the burning heat of the extremities was converted into a coolness that was rather alarming; the pulse had sunk in frequency to 90° but was full and steady. Gentle frictions were applied to the legs and feet, but they were not long continued, for the general warmth speedily returned; the heat in the trunk of the body rose in about an hour to 100° and the pulse to 100. His delirium went entirely off; the fur on his tongue speedily disappeared; and twenty hours afterwards he was found free of every complaint but debility.

Subsequent experience has however convinced me, that though in this case the termination was so happy, the cold affusion was not unattended with hazard. Sweating had commenced, and the heat was sinking. It had perhaps sunk more at the moment when the affusion was performed, than was indicated by the thermometer, for the bed cloaths often keep the body from cooling under sweating, to the degree that would otherwise be produced. In this case when the surface is suddenly exposed to the external air, the heat sinks rapidly. If the sweat had continued an hour longer before this remedy was used, the heat would have been still

still more diminished ; a torpor of the vessels of the surface, and of the extremities, would have been produced, followed by a great, and probably a dangerous re-action of the centre. This observation will be illustrated in the sequel.

In recommending the affusion of cold water as a remedy in fever, an express exception is therefore made against its use during the feverish chill, or after the sweat has begun to flow profusely, and more especially after it has continued to flow profusely for some time. An exception is also made against its being employed in the latter end of fever when the strength is much exhausted and the heat is sometimes as low or lower than the temperature of health. While, however, the heat rises one or two degrees above the health standard, this remedy may be used even in the latter stages of fever. I have employed it with advantage on the 11th, 12th, and 13th days. In instances of this kind it will however be prudent to make the degree of cold very moderate, as has been already observed ; and as it is scarcely to be expected, that at an advanced period of the disease the progress of it can be stopped, or its duration much lessened, it may perhaps answer every purpose to employ in such cases the *tepid* affusion. I have indeed often contented myself with spunging the body all over with tepid vinegar, or vinegar

and water, from the 9th or 10th day forwards ; but I have frequently in cases where the heat continued high, directed the general affusion of tepid water, by which the heat may always be speedily and effectually reduced, *when that is the only object in view.*

Under these restrictions, the affusion of cold water may be used with perfect safety in the low contagious fever of this country, and the facts already stated, will shew that it is a remedy of great power and efficacy. In the first stages of fever, it appears very generally to cut short the disease almost instantaneously ; and even when it fails of this effect, as is usually the case when it is applied in the more advanced stages, it nevertheless moderates the violence of the symptoms, and shortens the duration of the disease.

CHAP. VIII.

General remarks on fever. History of a case of fever in which the affusion of cold water was not salutary.

SINCE the introduction of scientific arrangements into medicine, diseases have been much reduced in number, and their nature has been more clearly understood. This is especially true of continued fever, which is exhibited by Dr Cullen under three genera only, Synocha, Typhus, and Synochus. Of these genera, however, the Synocha, or pure inflammatory fever, without topical inflammation, is confessedly a very rare occurrence in this island; the venerable professor used to declare that he had not met with a single instance of it in forty years practice.—And the Typhus and Synochus seem to be considered by him as the same disease, modified differently, by the difference of climate, season, and constitution. Both are described as contagious, and as occasion

ally producing each other. Doubtless the Typhus, or low contagious fever, is the prevailing fever of this island, and of Europe. It is the epidemic of all our great towns, of our jails, hospitals, and manufactories; its origin and progress are clearly ascertained, and its symptoms generally understood. It is to this fever that the preceding observations chiefly apply.

I have my doubts, however, after much reflection and observation, whether we have not simplified too far in our nosological arrangements of fever. The dreadful disease which prevailed lately at Philadelphia, and which now ravages the West Indies, (1798), cannot perhaps be included without some violence within our systems of Nosology; and its fatality under all the established modes of treatment, whilst it excites our deepest regret, must serve to abate the pride of modern science. Even in our own island, it appears to me that cases of fever sometimes occur, which cannot be referred with advantage to any of the genera of Dr. Cullen*. The following is a description of a fever of this kind;

* I am aware that all questions respecting nosological arrangement have a tendency to degenerate into verbal disputes, and I willingly avoid them, referring for my accuracy to those who have studied diseases, not in books only, but in the volume of nature.

kind; I have not met with it often, but when I have met with it, it has very generally proved fatal, under the established modes of treatment; and I am sorry to say, that in the only instance of this fever in which I have tried it, the affusion of cold water proved unsuccessful also.

The fever in question does not seem to originate in contagion, or to propagate itself by contagion. I have never been able in a single instance to trace it to that source; nor have I ever found it to be communicated from the patient to any of his attendants. The cases which I have seen have occurred chiefly in the winter season, in persons in the flower or vigour of life, possessed of considerable sensibility of mind, and in the habits of more than ordinary mental exertion. After some days of indistinct catarrhal complaints, the fever comes on (in general after some accidental exposure to cold) with a very violent and long continued attack of chills and rigor, and to this, as is usual, succeeds a state of heat and re-action. The patient complains of intense head-ach and of oppression at the præcordia, with occasional but not severe cough, and with some increase in the frequency of respiration. His pulse is not remarkable as to frequency or strength; his sleep is not particularly disturbed; and for some days the complaint goes on as if produced

duced by catarrhal fever. From the first, however there appears a great quickness and impatience about the patient: he talks more rapidly than usual; apprehends you quickly, and answers you instantly. He cannot, however, command his attention long, and is fatigued with the effort. His heat, which was at first moderate, becomes very great on the 7th and 8th day, reaching 107° or 108° of Fah'.; he becomes delirious and talks incessantly. Throughout the fever, his senses of hearing and taste are uniformly acute, and this is true also of his sense of feeling. Great as his heat is, he is much alive to the impressions of cold on the surface of the body and shrinks from them. At times he appears surprisingly calm and natural, gets out of bed and dresses himself, insisting that he is well. Often he starts up suddenly in bed and opens his curtains, seeming to look round the room for some person he supposes present; and sometimes he rings the bell violently, if within his reach, without apparent object. Indistinct conceptions rise and vanish in his mind, and the impressions of sense are confounded with the ideas of imagination. As the fever advances, the respiration becomes more hurried and laborious, the pulse more frequent and feeble; and towards the latter end of the disease, but not before, sweats break out, at first partial, and at length general and profuse, which, however, though they reduce the

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heat, do not otherwise relieve him. The pulse sinks; the body is covered with petechiæ; wine, bark, opium, and blisters, afford no relief: the patient dies on the twelfth or thirteenth day of fever, and after death the body runs rapidly into putrefaction.

I have seen this fever treated by venesection and antimonials in the early stages, with a strict attention to the antiphlogistic regimen; and by bark and cordials, as the strength began to decline; but without success. I have also seen it treated from the first on the same plan as typhus, but with an equally unfortunate issue. In a case of this fever which occurred lately, I made use of the cold affusion, and as the mercury rose in the thermometer with the bulb under the tongue, to 107° , I employed this remedy with some degree of confidence. The effects did not correspond with my former experience or with my hopes. The patient felt the cold most acutely, but was not relieved. His pulse did not diminish in frequency: his heat subsided very little, and that for a few minutes only; neither diaphoresis nor sleep followed. This remedy was not repeated, but the surface of the body was spunged from time to time with vinegar, without however producing sensible benefit or refreshment.

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I have already mentioned that the affusion of cold water is not to be used after a profuse perspiration has taken place; and that it is not to be used in the cold stage which begins the paroxysm of fever, nor till the hot stage be fairly formed. In the typhus, however, this last restriction seldom requires us to wait long; the affusion may be used in general in twenty-four hours from the original attack, and often much sooner. The case I have just related is the only instance, out of many hundred trials, in which I found, that even on the sixth day of fever, with the actual heat of the body far above the temperature of health, the affusion of cold water was neither salutary nor refreshing. I have however to observe, that notwithstanding the great heat of the body, producing the utmost restlessness and anxiety, the *sensation of heat* was interrupted by chilliness on the slightest application of cold, and that the surface and extremities not only felt chilly, but grew cold, even on the accession of the external air. In reality, through the greater part of the fever, the state of the patient had a considerable resemblance to what we see in the paroxysm of an intermittent, when the cold stage is terminating, but the hot stage not fairly formed—when the heat, as well as the blood, is accumulated in the centre of the system, and the vital power is struggling to give them that propulsion to the superficies, which terminates

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minates in profuse perspiration, and carries off the disease. I have little doubt that immersion in the tepid bath of the temperature that feels comfortably warm to the skin, continued for some time, is the proper remedy in the fever I have described, as it doubtless is in the struggle of the paroxysm of intermittent; and when an opportunity offers, I mean to ascertain the truth on this point. But this opinion will be illustrated when we come to speak of the warm bath more particularly.

If any one should contend that the fever I have described is in reality only a variety of the typhus, or synochus, I shall not be disposed to contest the matter. The question concerning *identity*, leads to endless disputes in every branch of science where it occurs, and he must know little of nosology, who supposes *it* has yet received a consistency, that would render such a discussion profitable. It is sufficient for me to observe, that the symptoms of the two diseases are in a considerable degree different, though with that general similarity that belongs to all cases of fever; that the state of the nervous system as to impressibility is widely different; and what is of most importance that the methods of treatment, which according to my experience almost invariably succeed in the one disease, are unsuccessful in the other. Every practitioner

practitioner knows, that in typhus, the sense of hearing is generally obtuse; and the same may be said of the taste, smell, and touch; whether the observation is applicable to the sight also, and under what restriction, appears to me doubtful. The acuteness of all these senses in the fever which I have described, is very remarkable, and particularly in regard to the sensibility of the surface. I have observed this symptom to be produced by several narcotics, and by some poisons. It is very remarkable in the hydrophobia; and in the last days of a person who died of inanition, the senses of touch and vision were extremely acute, and which will be detailed in the course of this volume.

CHAP. IX.

*Of the use of the affusion of cold and tepid water
in small-pox, with cases.*

THE singular degree of success, that on the whole attended the affusion of cold water in typhus, encouraged a trial of this remedy in some other febrile diseases. Of these the small-pox seemed more particularly to invite its use. The great advantage that is experienced in this disease by the admission of cool air, seemed to point out the external use of cold water, which being a more powerful application, might be more particularly adapted to the more malignant forms of small-pox. The result corresponded entirely with my expectation. Of a number of cases in which I witnessed the happy effects of the affusion of cold water in small-pox, I shall give the following only.

CASE I.

In the autumn of 1794, J. J. an American gentleman in the 24th year of his age, and im-

mediately on his landing in Liverpool, was inoculated under my care; the prevalence of the small-pox rendering it imprudent to wait till the usual preparations could be gone through, or indeed till he should recover from the fatigues of the voyage. He sickened on the seventh day, and the eruptive fever was very considerable. He had a rapid and feeble pulse, a fœtid breath, with pain in the head, back, and loins. His heat rose in a few hours to 107° , and his pulse beat 119 times in the minute. I encouraged him to drink largely of cold water and lemonade, and threw three gallons of cold brine over him. He was in a high degree refreshed by it. The eruptive fever abated in every respect—an incipient delirium subsided, the pulse became slower, the heat was reduced, and tranquil sleep followed. In the course of twenty-four hours the affusion was repeated three or four different times at his own desire; a general direction having been given him to call for it as often as the symptoms of fever returned. The eruption, though more numerous than is usual from inoculation, was of a favourable kind. There was little or no secondary fever, and he recovered rapidly.

In situations where the eruptive fever of small-pox is clearly distinguishable, and where it does not abate sufficiently on the admission of cold air, the

the affusion of cold water may be resorted to with confidence and safety, regulated however in this application, as in every other, by *the actual state of the patient's heat, and of his sensation of heat*. In the confluent small-pox, however, after the eruption is completely formed, this remedy cannot perhaps be used with advantage. The following case will illustrate this position.

H. A. aged 23, an American mariner, fell under my care (*Dec. 7,*) on the third day of the eruption of the small-pox; that is, on the sixth day of the disease. His pulse 114 and feeble, his heat 109°. His head, back, and loins, ached severely—thirst great—skin livid—small-pox confluent.

He was put on on a milk diet—gentle mercurial purgatives were ordered from time to time, and an opiate every night at bed-time. Lemonade was given largely, at first by itself, and afterwards mixed with wine, and the affusion of cold water was directed in the usual way. In ten minutes after the affusion, the pulse was 96, the heat 98°; the livor of the skin was much diminished, but the pains were not relieved.

Dec. 8. Noon—Pulse 96, soft and regular—thirst gone—respiration slow and natural—heat 97°. The affusion was ordered to be repeated; ten minutes after, pulse 84 and feeble—heat 84°.

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Dec. 9. Noon—Pulse 88, heat 93°—the cold affusion was not repeated in this very reduced state of heat; the decoction of bark was ordered, and a pint of wine daily in lemonade.

Dec. 10. Noon—Pulse 116, and full—heat 98°, respiration still easy—expectoration considerable, and viscid—thirst less—eyes quite closed—head swelled—a complete union of the pustules on the face.—Bark and wine continued, with the opiate at night.

Dec. 12. Pulse 118—heat 96°. A bucket full of water of the temperature of 92° was poured over him. He appeared refreshed at the moment; ten minutes after, pulse 112, heat 94°. Complained of being chilly. Respiration still easy—free of pains, and his face less swelled. Complained of his throat. A blister was applied to it all round.

Dec. 13. Noon—Pulse 118—heat 96°—respiration still free, but his throat very sore. Medicines were continued, but the affusion of tepid water was not repeated.

Dec. 14. Noon—Pulse 138—heat 100°—respiration had now become laborious, and the expuition difficult. The throat was much swelled. He
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was frequently spunged with tepid water, and the medicines continued.

Dec. 15. Noon—Unfavourable symptoms increased.

Dec. 16. Noon—Vomiting came on, which was relieved by opium. His senses and his intellect remained acute till within an hour of his death, which happened at eight o'clock in the evening of this day.

If this case be more detailed than seems necessary, let this be excused, as it is the first in which the actual heat in confluent small-pox has been recorded. It is here given accurately from the period when the disease came under my care.

In regard to the effects of the cold affusion, it may be observed that this remedy was not used during the eruptive fever, nor till three days after the eruption had appeared, and the character of the disease was decided. In the stage in which it was employed, the fever and the heat were abating, as is usual after the eruption; and in all cases in which the heat is sinking, the application of cold must be made with great caution, as has already been mentioned. After the second affusion (on the 8th) the heat sunk below its natural standard,

dard, and continued below it for some time; so that this remedy became inadmissible. The disease went through its usual course. The tepid affusion on the eighth day of the eruption (*Dec. 12*) was used in part to wash off variolous matter, and in part to produce refreshment. The heat, which was before 96° , sunk two degrees, so that it could not with safety be continued, for experience has proved, that the tepid affusion is a powerful means of diminishing heat. The heat rose again with the secondary fever, and the patient died of the affection of the throat, as I believe is general in the confluent small-pox.* It will be at once perceived, on the principles already laid down, that in a disease like this, the affusion of cold water could only be essentially useful during the eruptive fever. It is during the eruptive fever that the quantity of the assimilation is determined, as well as its kind. This is, I believe, invariably found to bear an exact proportion to the eruptive fever, and whether we consider the eruptive fever as the cause or effect of the assimilation, there is every reason to expect, from the laws of the living system, that the diminution of this fever will diminish the quantity, and meliorate the quality of the variolous eruption.

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* See *Zoonomia*, vol. ii. page 237.

In the case just related, the heat during the eruptive fever (judging from trials in similar situations) had risen to 106° or 107° ;* but it had sunk to 100° , before the cold affusion was employed. It may easily be conceived that this remedy could have been employed to a much greater extent, and that its effects would have been far more salutary, if it had been used throughout the previous fever. That it would have essentially altered the character of the disease, I presume not to assert. This however I can declare, that in all the cases in which I have used the affusion of cold water during the eruptive fever, however severe the symptoms may have been, these symptoms instantly abated, and the disease assumed a benignant form. The case of Mr. Johnston (Case I.) already given, will illustrate this observation; and six or seven others I might adduce to the same purpose. As yet my experience extends no farther.

The inoculation of infants is so very rarely followed by any serious disease, that as far as respects them, the affusion of cold water may be seldom required. The inoculation of adults is not, however, quite so safe. Inoculation is seldom

* 1803. I now believe that the heat does not rise so high in any stage of confluent small-pox. See the *Additional Reports*.

dom indeed performed in our island on adults that are natives, but foreigners frequently require it: and in Liverpool, our intercourse with America renders it often necessary to perform it on adults from that continent. We may also observe, that when the natural small-pox is epidemic, the eruptive fever will be generally distinguishable, and wherever it is distinguished with symptoms of violence, instead of trusting to cool air only, the cold affusion, or cold bath, is strongly recommended. To our brethren across the Atlantic this is more especially addressed.—In America, as well as many parts of the old continent, in consequence of the neglect of early inoculation, the natural small-pox at times spreads alarm and devastation throughout extensive districts. In this island the ravages of the natural small-pox are on the whole very great, yet they occasion little disturbance or alarm. The practice of inoculation among the more opulent classes of society, keeps up the contagion in all our populous districts, and at the same time by relieving these classes from the apprehension of the natural-pox in their own families, prevents them from opposing the casual progress of the disease among the inferior orders, who want the knowledge and the combination necessary to the use of the means of prevention among themselves. Though therefore it is demonstrable, and has indeed been demonstrated, that the destruction of

the natural, or rather the casual small-pox, might be entirely avoided, yet, as it falls almost wholly on the families of the poor, and as it has been an evil that has been long, and that is familiarly known, we submit to it through habit, as if it were inevitable.* The alarm produced by the casual

* The observations on the possible prevention of the casual small-pox, will doubtless suggest to the medical reader, the "Sketch of a Plan" for that purpose, published in 1793, by my respectable neighbour, Dr. Haygarth. That this plan is in itself practicable, and that it would be effectual, I have little doubt. Unfortunately, it requires the assistance of government (as I remarked at the time) and this I fear is a powerful objection. It is possible however that some scheme of this kind (including I hope all contagious diseases) may one day or another be attached to some comprehensive plan for the management of the poor.

The theory that suggested Dr. Haygarth's plan, but which formed no essential part of it, involved him in a discussion on the length of time that variolous matter may be exposed to the atmosphere, and retain its infectious quality. On this occasion, as there was some difference of opinion between us, he proposed several experiments to me, which would doubtless have decided the question. (*See p. 459, 460, of "A Sketch, &c."*) It was fully my intention to have undertaken some experiments such as he mentioned, and I even commenced them, but as my attention was forcibly drawn to other subjects, these experiments, which required extreme accuracy, were not completed, and my engagements have never since per-

casual contagion is therefore seldom so great in our large towns, as to give a practitioner frequent opportunities of treating the eruptive fever of the confluent small-pox, the only stage of that disease in

mitted me to recommence them. I have, however, since that time, inoculated with matter at different periods from its being taken from the patient, and the result is as follows :—The length of time which variolous matter exposed to the air retains its contagious quality, depends on its superficies. If it be spread very thin on a piece of flat or convex glass, it loses this quality much sooner than when it is collected in a mass. Spread thin upon glass, it sometimes disappoints the inoculator at the end of twenty days, though not generally ; and I have known it succeed in communicating the disease, even when diffused over a large surface, at the end of seventy-three days. (*See Dr. Haygarth's Sketch, p. 447.*) But I find from the experience of others, as well as from my own, that this is not common. The instance in which I found variolous matter to retain its infectious quality longest, was the following : On the second of February, 1792, I took a considerable quantity of this matter on a piece of window glass, keeping it as much together as its fluidity would admit. It was exposed immediately to a stream of air, and the surface was speedily dry. On the second of March following, after moistening a portion of it with a little water, I inoculated three patients, and all with success. In the course of the summer I inoculated with another portion of it, previously liquified by the addition of warm water, and with success as before. On the twentieth of July, 1793, I again used a portion of the same matter in the same way, and again with success ; but this success was long doubtful, and it was not till the twenty-second day after the operation, that the patient sickened.—I used the

in which medical treatment is likely to be of much avail. Where such an opportunity does occur to the judicious reader of these pages, it is hoped that the affusion of cold water, or the cold bath, will not be neglected. The Chinese, it is said, have long followed this practice with extraordinary success*.

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same matter in June, 1794, when it entirely failed me. It is now by me, and is not mouldy, nor any ways changed in its appearance. Variolous matter kept some time is certainly slower in producing the disease, even where it does succeed in the end.

1803. The happy discovery of the certain method of preventing, and of finally annihilating the small-pox, given to the world by Dr. Jenner, deprives experiments of this kind of much of their utility and interest.

* Sir William Watson, in his tract, entitled "Account of Experiments on the most successful methods of inoculating the small-pox," published in 1761, mentions the case of a young woman, who in the absence of her nurse, got out of bed delirious during the eruptive fever of small-pox, and threw herself into the new river near Islington. She was discovered floating on her face; and when taken out of the water, had not the least appearance of life. She was recovered however by the usual methods, and afterwards passed well through the disease.

In some parts of Bengal, according to Mr. Ives, inoculation is practised among the natives—After the operation is performed, the patient is ordered to bathe in cold water thrice

The use of the cold affusion in fever was common among the physicians of the infirmary, (Dr. Brandreth, Dr. Gerard, and myself) while fevers were received into that hospital, and its extraordinary success there, has rendered it in Liverpool familiar in private practice, and extended it, as I have already mentioned, to several other febrile disorders. The scarlatina anginosa, has for many years prevailed in Liverpool, and though in general mild, has, as at particular seasons been malignant and fatal. To this disease, the affusion of cold water has lately been applied by my friend Dr. Gerard, and the particulars that follow are mentioned on his authority.

In the latter end of December 1796, all the children of a family in his neighbourhood, five in number, had been attacked in succession with scarlet fever; four of these were recovering, but one was dangerously ill, when the father of the family, with whom one of the children had slept, was himself seized with all the symptoms of the disease

a day, and to live on the most cooling diet. When the fever comes on, the bathing is left off, but it is resumed on the second day after the eruption appears, and continued for the three successive days. It is supposed to fill the pustules.

Ives's Voyage to India, in the years 1755, 6, 7, ch. iv. p. 54.

disease. He had excessive pain in his head and back, and flying pains all over him. He had frequent rigors, loss of appetite, and sickness, with some flushing of the face, but without any efflorescence on the skin, or affection of the throat. This was his situation when Dr. Gerard was called in, about sixteen hours after the first attack. An emetic, and afterwards a cathartic were ordered, but their operation was slow and imperfect, and on visiting him ten or twelve hours afterwards, he was not materially relieved.

Entertaining no sort of doubt of the nature of the attack, and these symptoms foreboding that the epidemic would in this instance be severe, Dr. Gerard determined to try the affusion of cold water, from which in typhus he had seen such happy effects. Accordingly the operation was performed, and with a result that far exceeded his hopes. As the patient was much debilitated, half a pint of hot wine was given him after it, and on being put to bed, the symptoms of fever were found nearly gone; a genial warmth diffused itself over the extremities of his body, followed by sensible perspiration and sleep. Next day he complained of a slight degree of head-ache and lassitude; Dr. Gerard therefore ordered the affusion to be repeated, as well as the warm wine
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after it; the symptoms of the disease vanished, and never re-appeared.

A day or two afterwards, a maid who had been hired as an assistant to attend the sick children, and who had been about a week in the house, was attacked by the precise symptoms already related, and which had uniformly ushered in the epidemic. She took an emetic on the first attack with little benefit, and soon after the cold water was poured over her, the wine being administered after it. In this case the remedies were used earlier than in the former one;—they were used once only: the febrile paroxysm was dissolved, and never returned.

The result of these cases communicated by Dr. Gerard, leads to a variety of important reflections. That the affusion of cold water extinguishes the incipient scarlatina as well as the typhus, can scarcely be doubted; and thus this powerful and simple remedy is extended to another, and a most important class of diseases. That the disease may be extinguished without the specific efflorescence of the skin, or affection of the throat, is a circumstance not a little curious. It seems to demonstrate that this efflorescent matter is the product of the eruptive fever; and that the fever itself being destroyed in the first instance, the efflorescent

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matter

matter is never produced. Thus we are freed from the apprehensions which a false theory might suggest against extinguishing a process by which nature was extricating itself from an acrimony which the system had imbibed.—Thus also our conclusion is supported, that the eruptive fever of small-pox is the cause, and not, as some have supposed, the consequence of the progress of assimilation, and that the diminution of this fever by cool air, and still more by the affusion of cold water, actually diminishes the quantity of matter assimilated, and in certain cases might perhaps wholly prevent the assimilation.* This last conclusion is indeed doubtful, as all analogical inductions must be between different diseases; and particularly where the difference is of this important kind, that the constitution in one case is susceptible of the disease once only, while in the other it may probably receive it indefinitely.† We may also

* Dr. Cullen has assumed, that in all the various degrees of small-pox, the quantity of matter assimilated bears an exact proportion to the bulk of the body, and that the difference in the nature and quantity of the eruption depends wholly on the permeability of the skin. This doctrine, which in his lectures he extended to all the other exanthemata, is in my judgment, one of the weakest parts of his most valuable work.

† (1803) I was of opinion that the same person might be again

also observe, that the prevention of the assimilation of small-pox, by wholly extinguishing the eruptive fever, if it were in our power, would not be advisable, since it must leave the patient exposed to the future influence of that contagion.

I have not had an opportunity of repeating Dr. Gerard's practice in the incipient stage of scarlatina, but after the efflorescence on the surface decides the nature of the attack, I have for the last fifteen months uniformly prescribed immersion in the tepid bath, (from 92° to 96°) and with striking benefit. Whether the affusion of cold water is applicable to the other exanthemata, must be left to future experience.

In the cases that I have related to illustrate the effects of the cold affusion, the temperature of the water may be judged of from the season of the year. In general it was from 40° to 50° of Fah°. In the epidemic which prevailed in the 30th regiment, the water of the river was employed, as has already been mentioned, which as the season was uncommonly cold, did not,

again and again affected by scarlatina, but experience leads me to a different conclusion. I now believe that scarlatina, like small-pox and measles, affects the same person once only. In the *Additional Reports*, Vol. II. the reader will see an ample detail of my farther experience of the nature and treatment of this disease.

though in the month of June, exceed 58° or 60° . I have, however, very often used the river water in private practice during the summer months of the last four years, when in general it has been from 65° to 70° , and the effects corresponded with those already described. The solution of fever depends chiefly on the sudden, general, and powerful impression on the sensations, and this impression is less affected by the difference in the temperature from 40° to 65° , as far as my observation extends, than might on a first consideration be imagined. Within these limits the efficacy of this remedy, as well perhaps as its safety, depends on the suddenness and momentariness of its application. The powerful impression on the sensations is much weakened when the water is poured slowly on the body, and as the respiration is suspended or convulsed during this application, as well as during the act of immersion in the cold bath, it might in some cases incur hazard to protract it.

CHAP. X.

Of the affusion of tepid water on the surface of the body, in feverish disorders, and of spunging the body with water or vinegar. The affusion of tepid water practised by the ancients.

I APPLY the term *tepid* to water heated to that degree which is warm but not hot to the sensations, and which in the way of affusion is from 87° to 97° of the scale of Fah'. According to my experience, this term, when the body is immersed, may be applied to water some degrees colder; the reason of which will be easily understood by those who reflect, that under immersion no evaporation from the surface of the body takes place. At first I imagined that the tepid affusion might be beneficial in cases where the heat of the body is below the degree necessary to render the cold affusion safe. I employed it therefore in those stages of fever where the heat did not exceed the temperature of health. A little experience however convinced me that this practice required strict attention, for I found, that in many cases, at least, the heat of the living body

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is lowered as speedily by the affusion of tepid water, as by the affusion of water that is cold:— if I mistake not, in some cases the heat is lowered more speedily by the tepid water. To those who reason respecting the heating and cooling of the living body in the same manner as respecting inanimate matter, this observation will appear paradoxical: I assert it however from actual observation, and a little reflection will explain the phenomenon. The evaporation from the surface is more copious from the tepid affusion, and on this the cooling of the body very much depends. But this is not all; the tepid affusion is little if at all stimulating, and does not, like the cold affusion, rouse the system to those actions by which heat is evolved, and the effects of external cold are resisted. Where the object is to diminish heat, that may be obtained with great certainty by the repeated use of the tepid affusion, suffering the surface of the body to be exposed in the interval to the external air—and if the beams of the sun are excluded, and a stream of wind blows over it, the heat may thus be reduced where cold water cannot be procured; even in the warmest regions of the earth—on the plains of Bengal, or the sands of Arabia. I have accordingly employed the tepid affusion very generally in those feverish affections where the morbid actions are weakly associated, depending rather on the stimulus

of preternatural heat, than on contagion, miasmata, the morbid contents of the stomach and bowels, or local inflammatory affection. Of this kind are a great part of the feverish affections of children, in which the tepid affusion is a valuable remedy. It very generally produces a considerable diminution of heat, a diminished frequency of the pulse and respiration, and a tendency to repose and sleep. I have used it also in feverish disorders of various kinds were the lungs are oppressed, and the respiration laborious, and where of course the oppression might be dangerously augmented by the sudden stimulus of the cold affusion. It is also applicable to every case of fever in which the cold affusion is recommended, and those may receive much benefit from it, whose fears or whose feebleness deter them from that energetic remedy. I have not however found its effects so permanent as those of the cold affusion, and I have never seen it followed by the total cessation of regular fever, as often occurs after the cold affusion. In the hectic fever, however, where the actions are less strongly associated than in synochus or typhus, the paroxysm is sometimes completely extinguished by the affusion of tepid water* on the commencement

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* See this fact mentioned in the Zoonomia, vol. ii. p. 296, where a relation is inserted by Dr. Darwin, of the author, (Dr. C's.) own case of hereditary consumption

mencement of the hot stage. In the hectic paroxysm, the heat seldom rises more than two degrees above the temperature of health in the trunk of the body, and three or four degrees on the extremities. By moistening the palms of the hands and the soles of the feet with vinegar, its effects may be moderated, for it is from the sensation of heat in the extremities, that the stimulus to the system is chiefly derived; and this practice ought not to be neglected, if the tepid affusion is not employed generally. In all cases of fever indeed where the burning heat of the palms of the hands and soles of the feet is present, this method of cooling them should be resorted to; it is uniformly safe and refreshing. I have not employed the cold affusion in the hectic paroxysm. This disease generally adheres to a debile system; the body parts with its heat in it easily; and the lungs being always affected in the pulmonary hectic, the sudden application of cold to the surface might produce unpleasant, and perhaps dangerous effects on the respiration. Neither have I tried it in peripneumony, or measles*.

When the affusion of water, cold or tepid, is
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* (1803) The reader will find some account of its being accidentally used in a few cases of measles, in the *Additional Reports*, vol. ii.

not employed in fever, benefit may be derived, as has already been mentioned, though in an inferior degree, by spunging or wetting the body with cold or warm vinegar or water. This application is however to be regulated like the others, by the actual state of the patient's heat, and of his sensations. According to my experience, it is not only less effectual, but in many cases less safe : for the system will often bear a sudden, a general, and a stimulating application of cold, when it shrinks from its slow and successive application *.

I have also used the affusion of *cool* water as a
remedy

* I have purposely avoided entering on the general operation of cold, in hopes of being better prepared for the subject at some future opportunity. In the mean time I have continued the use of such terms as are most intelligible, and most consonant to the true doctrines as far as I perceived them. To speak of cold in any form acting as a stimulus, seems however to some learned friends, not merely an error, but an absolute contradiction in terms. Heat, they affirm to be the universal stimulus, and cold being merely the privation of heat, *must* in their opinion, always have a sedative operation. As well, it is said, may darkness, which is the privation of light, stimulate the eye, as cold, which is the privation of heat, stimulate the general system. This observation, which has more importance with me from the quarter whence it comes, than from any intrinsic weight it possesses, leads me into one or two general remarks.

remedy in febrile diseases, but more frequently in paralysis, and in other diseases of debility. By the

That cold can never act as a stimulus, was a favourite dogma of the late Dr. Brown. It was a necessary consequence of his general doctrines of life. As he admitted only of two classes of diseases—diseases of increased and diminished excitement; so he reduced all remedies to two corresponding classes—such as diminish, and such as increase this excitement. That cold in extreme degrees is a powerful and *effective* sedative is incontrovertible; he therefore held that it is sedative in every degree, without being at the trouble to point out the line of temperature below which the term *cold* is applicable. It was the character of Dr. Brown to follow his hypothesis into all its consequences, contemning all facts that rested on the testimony of others, and neglecting more than perhaps any man of his talents ever did, to bring his opinions to the test of experiment. His theory was in his hands the bed of Procrustes. On the operation of cold he was singularly erroneous. He asserts that cold can never be of service in the fevers of the torrid zone: (*Elements of Med.* vol. i. p. 23.) and the reason he gives is, that in those diseases there is indirect debility, to which cold must not be applied. He declares that it never can be of service but in sthenic (inflammatory) diseases, (vol. i. p. 103.) that for these it is the grand remedy; (p. 296) that its operation is the same in measles, catarrh, and all other inflammatory diseases, as in small-pox; and that “it is sufficient to cure them all!” These assertions are not merely erroneous, they are dangerous in a high degree.

Whatever opinion may be entertained of Dr. Brown's fundamental principle—that all the phenomena of life are to be explained, by assuming, that the living principle (the

the term cool, I indicate the temperature from 87° to 75°. It operates as a gentle stimulant, and may be used as a milder form of the cold affusion.

excitability) is accumulated and expended in the inverse ratio of the stimulation, it will not now be disputed, that he was acquainted with only one mode of action of the living principle, that which Dr. Darwin has described under the name of irritation; and that he was wholly regardless of the influence of sensation, volition, and association, since his time so fully illustrated in *Zoonomia*, and which on every hypothesis must be allowed to have such vast influence on the motions of life. It was to his disregarding the action of cold on the sensations, that the error of Dr. Brown is more immediately to be traced.

It is not easy to explain why Dr. Darwin, who understood the laws of sensation so well, should also have disregarded the influence of cold on the sense of feeling. He seems to consider the application of cold as merely subtracting the stimulus of heat, and thus to be altogether negative; (*Zoonomia*, vol. ii. p. 757.) without adverting to that most powerful sensation, by which great and sudden changes from a higher to a lower temperature are attended, through which the energies of life are roused into action, and the sedative influence of cold for a time counteracted. Whatever theory we adopt in regard to the origin of caloric in the living body, it seems to me to be in a great measure owing to the influence of the changes of the external temperature on the sensations, that the uniformity of the heat of the animal is preserved; and this view of the subject is, as far as I can see, consistent with the general doctrines of Dr. Darwin.

affusion. Like the cold affusion its application should be sudden and momentary, when the object is to increase the tone of the system, or to dissolve

The stimulating action of cold, though short in duration, is powerful in degree. In the torpor of convulsion, when weaker stimuli are unperceived, the affusion of cold water on the naked body will often excite the dormant sensibility, and introduce a new action throughout the nervous system. In the apoplectic state brought on by the fumes of charcoal, this remedy is of all others most efficacious; when dogs are suffocated in the vapour of the *Grotto del Cani*, it is well known that they are recovered by plunging them in the adjoining lake. And in other animals, when the last motions of life are apparently over, the same application will sometimes, as I have observed, renew the contraction of those fibres that seemed before fixed in death.—This observation I hope to illustrate at some future opportunity.

Can darkness stimulate the eye? No—Darkness produces no sensation. Can cold be rendered stimulating? Surely—After what has been observed no one will deny it; unless indeed it should be said, that it is not the cold that stimulates, but the sensation which the cold produces; a point that it would be a waste of time to dispute. It is the stimulant power of cold that renders it so difficult to employ it in inflammatory diseases. (See note at the end of chap. xv.)

(1803) The above note was written in answer to some observations in a letter from Dr. Darwin.—He expressed an intention of noticing it in the next addition of his *Zoonomia*, but nothing of the kind is to be found there.

dissolve a morbid catenation; where it is employed to moderate inordinate heat, it may be used more slowly, provided it does not interrupt the catenation on which respiration depends*.

The practice of giving cold water as a drink in fevers, was common among the ancients, and immersion in cold water they occasionally employed; but the affusion of it on the surface of the body seems to have been in a great measure unknown. Ablution of the surface with cold water in feverish diseases, has been traced under different modifications among the practices of the rude nations of Asia and of Africa by modern travellers, particularly by Sir John Chardin, and Mr. Bruce; but it was first brought into notice in Europe during an epidemic fever which prevailed at Breslaw, in Silesia, in 1737, as appears from the dissertation of J. G. de Hahn, under the title of *Epidemia verna quæ Wratislaviæ, anno 1737, afflixit*; to be found in the Appendix to the

* I borrow this term (catenation) from Dr. Darwin—It is employed to express a number of actions linked together, nearly in the sense of *association*: the *sympathies*, as they are usually called, are included under this term, and many *associated* actions to which the word sympathy has not been applied, as the connexion between the heart and the lungs, the stomach and the heart, &c. (See *Zoonomia*, vol. i. sect. xvii.

Acta Physico Medica Germanica, vol. . x.*
 While the laws by which the affusion of cold water ought to be regulated were not understood,

* De Hahn says that he is the better able to describe this disease, as he himself was seized with it; and was cured by "*peregrina illa multis visa medendi methodus.*" He begins with an account of the causes of the epidemic. That first mentioned is the state of the weather. In the month of May 1736, after a very promising spring, the weather became wet and cold, and the fall of rain was so considerable during June, July, and part of August, as to lay a great part of Silesia under water. The wind blew chiefly from the North-West.—The harvest was almost entirely destroyed. In consequence a famine prevailed throughout the province, of which the author gives a dreadful account, and this he mentions as the second cause. The third cause was the vitiated air. The unburied carcases of various animals were suffered to putrefy in the atmosphere, and the stagnation of the waters in the low grounds filled it with marsh effluvia. Even the cattle suffered severely from this state of the air, and from the want of pure water to drink. The epidemic began in the ensuing spring. He gives a number of cases. The first is that of a farmer in the neighbourhood of the city, of forty years of age. "*Comites febris erant subitus virium lapsus, capitis & præcordiorum dolor gravis, fluxio alvi, pervigilium, inquietas, delirium. Quæ insuperabilia morbi symptomata excipiebat die undecimâ his ipsis insuperabilia obitus.*" Many of the inhabitants of the country died in this way. The second case he details occurred in the city. It is that of a delicate woman, of thirty years of age, previously afflicted by severe misfortunes. The symptoms were as follows. "*Subito clanguida fiebat. Dolor capitis illico atrox. Sitis, & secundo die, importuni menses. Exin vomitus bilis, alvusque biliosa.*"
 Sputa

stood, the use of the remedy must have been extremely hazardous, and the fatal consequences of

Sputa viscida. Syncopiticus rigor. Ardor partium internus. Linguae, quasi candenti ferro compressæ, sicca glabrities & restrictio. Vox arentibus faucibus nulla. Angina sine tumore pessima. Repetebant interdum tenacis pituitæ vomitus alvi-que fluxio. Os sine medellâ siccum. Lactatio. Supinus torpor, Mors inter convulsiones gemibunda." The author proceeds to detail four other cases similar to the above, which also terminated fatally. Two females who caught the infection escaped in consequence of critical eruptions. The epidemic spread more and more.

At length the alarm became general. The attention of the magistrates was roused, and from a strict enquiry it was found, that more than double the usual number of deaths had occurred at Breslaw in that year. All the usual methods of practice were of no avail. Whether bleeding was employed or abstained from, the disease was equally mortal. In this distress Dr. De Hahn determined to try the effects of the external use of cold water. The first case in which it was employed, occurred in the month of April; it is related as follows. "*Mercator xxxii annos natus, floridus naturâ, mox febriens, capitisque dolore & nausæ affectus. Permittentibus id viribus, alterâ die mittebamus sanguinem, qui inflammatorius. Vespere vomiebat æger, tertiâ die maculosus. Maculæ morillis æmulæ, inconstantes. Mador. Mox præcordiorum intolerabilis dolor & delirium. Quasi ex temulentâ vacillans extra lectum vagabatur æger. Oculo altero dilatato, altero connivente, utroque granioso & caliginoso; linguâ lævi, arida, candente; sputo resinoso; urinâ biliosâ.*"—The usual remedies failing, on the eighth day they had recourse to ablution. "*Tum ad externas illas humectationes confugiebamus, indefessâ operâ spongiis*

of its improper application, we can easily believe to have prevented its gaining any ground on

spongiis omnem corporis ambitum demulcentes. Id consequemur, ut sensim sputa resinosa fierent magis mobilia; ut flaccida et fusca facies conspiceretur magis turgidula; ut *felicius procederet blandus mador*; ut resipisceretur, hactenus vel loquax nimium, vel taciturnus ex delirio æger.” The author then relates a case of a similar nature, in which the ablutions being rejected, the patient died. Next follows the case of a lady, a relation of his own, forty-three years of age, whose symptoms appeared of the most dangerous nature. In this case the attack was sudden. “Inter domesticas occupationes quasi conquassata concidebat. Facies primo momento Hippocratica; artubus glacie perfusis & tremulis. Horror repetitus ad quemvis æris attactum. Vomitus post quævis ingesta. Abdominis turgor. Videbatur mihi advolanti non morbo corripì ægra, sed siderata mori, vixque amplius cum morte colluctari. Pharmaco, ob vomitus, non erat locus; neque enæ sectioni, ob algida & emarcida membra. Die 2dâ, jugi ablutione artuum glacies dissuebat inter gemibundos angores. Sitis tandem invitabat potum. Calorem excipiebat æstus. Clysma solvebat partes abdominis internas. Quartâ die, magis sibi conscia ægra caput quasi a percussione dolere conquerabatur. * * * Octavâ demum die, certa reditura sanitatis spes, duorum tandem mensium confirmata..”

The author next relates the case of a man of sixty-four, whom he attended, with two other physicians, and on whom almost every remedy but ablution with cold water was tried. This person died on the tenth day, and our author himself was immediately after seized with disease having probably caught it from this patient. He was

on the continent, or its having been adopted in Britain. These laws are now, I hope ascertained

in his 44th year. "Die 1mo, Persenseram inter vix superabiles labores, infestum aliquamdiu nuchæ dolorem. Hic isto mane solito ferventior, febrem epidemicam invitabat absque prævio horroris sensu. Sævus ille dolor ex nuchâ ortum ducens—totum tunc ambiebat caput. * * * Fervebat caput, frigentibus pedibus, spasmo ad abdomen rigidis. Augescente de momento in momentum dolore, horror aderat ad quemvis aëris attactum. Languor mox ab initio extremus. Nox inquieta & sudans. 2do, Oculi gravissime dolentes * * *. 3tio, Parca mane remissio dolorum, mox intensiorum; febre, prævio tempore, auctâ. Nox clamoso nuchæ & capitis cruciatu atrox. 4to, Post hanc dies nocte pejor. Pedum glacies nondum egelata." These symptoms encreased daily. On the seventh day there was great subsultus tendinum, and the whole body was covered with petechiæ. In these circumstances recourse was had to ablution with cold water. "Ab hac die ablutio frigida universalis in usum vocata cum antea particularis tantum obtinisset. 8vo, Pulsus tremulus observatur. Gemitus a dolorum ferociâ perennis. 9no, Vomitus grumum cruoris emoliebatur, 10mo, Obtinebat tunc rerum ab aliquo mentis deliro stupore incuria. 11mo, Sudor, armistitium aliquot horarum producens, opportuno corticis usui locum concedebat, cum jam exitialis languor quævis pessima minaretur. Loquela difficilis & balbutiens. Angina aphthosa. Stridor dentium. 12mo, Risus sardonius. Spasmi cynici. * * * Id interim consequabamur corticis usu, ut quotidiana exacerbatio tardius, & die decimâ quartâ, quæ tota comatosa fuerat, sub noctem demum ingrederet. Sed gravitate tarditatem compensabat, violenti frigore totum corpus quatiente. Mox algidus aderat sudor, deficiente

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loquelâ ;

ascertained by ample experience, and practitioners will, I trust, find themselves directed in safety

loquelâ; demissa citra voluntatis imperium urina." From this apparently desperate state, Dr Hahn, however, recovered, and chiefly, as he apprehends, by the ablutions of cold water, which were employed even in this advanced stage of the fever. Almost from the beginning of the fever he indulged freely, according to the plan he had followed with others, in liquids, consisting chiefly of water, rendered grateful and cooling by the addition of lemon juice, nitre, &c. and every night he took a moderate dose of opium, from which he found the greatest benefit. From all heating alexipharmics he entirely abstained, having the greatest repugnance to them. On the seventh day, the general ablutions with cold water were begun. The method employed seems to have been that of sponging the surface, and this appears to have been continued for some time together. The effects are described as follows. "*Sanctæque testor, nunquam non refici ad breve temporis momentum languentes marcidæ cutis fibras me persensisse. Succedebat, repurgatis hac ope poris, perspiratio liberior, ac sudor modestus; videbanturque cutis obturacula promptius spongiâ remota, quam pharmaci interni virtute. Recreabat præterea hiantia & purum aërem sitientia cutis ora, liberior aëris accessus.*" This account of the effects of the practice, corresponds with our experience at present. Dr. De Hahn found the advantage of a plentiful supply of fresh air, at that time little understood throughout Germany or Europe, and he secured it by keeping his windows open. He changed his bed-clothes and linen frequently; and he occupied one bed during the day, and another during the night. a practice which moderated the heat, and often produced great

safety in the future use of this powerful remedy.

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great refreshment. This practice deserves indeed to be followed whenever it can be done with convenience. Experience convinces me that it always produces much comfort under feverish heat and irritation, and that it often procures tranquil sleep. Where a change of bed cannot be obtained, the method of Dr. Franklin, of exposing the body to fresh air and ventilating the bed-clothes, ought to be resorted to. De Hahn took the bark on the 11th day, and found it very grateful to his stomach, and agreeable to his sensations. That day he took five drachms of the powder. On the 12th, he took four drachms; on the 13th, three only; and on the 14th, only one; his relish for it having gone off. The bark seemed to moderate his symptoms, which returned with violence on leaving it off, and on the 15th day he found himself on the point of death. At this very time however he persisted in the cold ablutions, and to them he ascribes his recovery. "*Juges illæ ablutiones, quibus hucusque recreatus fueram, eâ ipsâ die, quâ mori videbar, non negligebantur; licet enim totus algidus algido sudore perfunderer, non secus ac liquefactæ glaciei immersus, frigidâ tamen abluebar; maritum serviturâ charissimâ conjuge lethales madores ocius deluente.*" From this time the symptoms became less violent, but his recovery was slow and difficult. On the 18th day he was still delirious, and syncope came on when he attempted to get out of bed. His appetite, however, began to return, he had copious sweats and fell into profound sleep. On awakening he felt a great aversion to noise, and every thing appeared new and extraordinary. On the 36th day cholera came on, but was subdued. On the 48th there was a desquamation of the skin, and a falling off of the nails. He concludes this account of his own case in the following words, "*Morbi tandem reliquæ menstrua equitatione feliciter subactæ.*"

The affusion of tepid water in febrile disease was not unknown to the ancients, though seldom employed

De Hahn was attended by his father, also a physician, under whose direction the ablutions were performed, and by whom the greater part of the symptoms must have been recorded. In consequence of this success cold ablutions were employed with others, and many were saved by them in circumstances apparently the most desperate.

It is evident from this abstract, that De Hahn was not regulated in his use of external ablution with cold water, by rules similar to those which I have ventured to lay down from several years experience. Instead of pouring the water over the naked body, he applied sponges soaked in cold water to every part of the surface in succession, and seems to have continued the application for some time together; in my judgment the least efficacious, as well as the most hazardous manner of using the remedy. He does not seem in general to have used the ablutions till the eighth or ninth day of fever, and till the cases were growing desperate from the failure of other means. At this advanced stage, the ablutions, as might be expected, seem to have been of very inferior efficacy. Yet in the single case, in which from the impossibility of the patient's swallowing medicines, ablutions were used on the second day of the fever, the recovery was speedy; it appeared certain on the eighth day; and this might have encouraged an earlier trial of the same practice in other instances. But what appears to me most surprising, is, that he does not seem to have been regulated in the use of this remedy, either by the actual heat of the patient or his sensations of heat. In his own case he expressly declares, that the cold ablutions were used on the 15th day of the fever, when he was shivering with cold, and covered with cold sweat, circumstances under which I should pronounce

employed by them.* It is, I apprehend, new in modern practice, and will be found an important addition

pronounce it to be in the most extreme degree dangerous. Whether my restrictions as to the use of the cold affusion, and the application of cold in general to the body, be too severe, future observations must decide; but from a general review of the incautious practice of De Hahn, I am not surprised that this boasted remedy, is, so far as I can learn, no longer in use, either in Silesia or in any part of Germany. Besides ablution, clysters of water were found useful in this epidemic, and water was used copiously as a drink. Eruptions of a supposed critical nature frequently appeared. This fever carried off three thousand persons in Breslaw and its vicinity. The means of making this abstract have been furnished me by a friend at Edinburgh, who copied for me the greater part of the memoir of Dr. De Hahn, from the 10th vol. of the *Acta Physico-Medica Academiæ Cesareæ, Leopoldino-Carolinæ, Naturæ Curiosorum Exhibentia*, in the College Library.

(1803.) In my second edition this note was printed very inaccurately, owing to an accidental circumstance—the quotations from De Hahn are now I hope given with sufficient correctness. The abstract of this dissertation in Sauvages is imperfect and incorrect, not being taken from the original work of De Hahn, but from the Review of it in the *Journal de Médecine* for 1757.

See *Nosologia Method*: Tom. i. p. 334, 4to edition.

* Some traces of its use in fever are to be found in Celsus.
 “ Quidam ex antiquioribus medicis, Cleophrantus, in hoc ge-
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addition to the list of our remedies. The effects differ considerably from immersion in the tepid bath, as will be more fully explained in speaking of that remedy. Though the affusion of tepid water was little in use as a remedy, it was familiarly practised as a part of their daily regimen, by the Greeks and Asiatics, in the earlier periods of their history. That the Greeks in later times, and after them the Romans, immersed themselves in the baths to which they were so much devoted, is certain; but in the heroic age of Greece, the affusion of tepid water seems to have been the mode in which this luxury was enjoyed. Abundant proofs of this are to be found in Homer, particularly in the *Odyssey*, that admirable record of ancient manners. Venus, after her public disgrace, is described as flying to the groves of Paphos, where she is *laved* by the Graces, and the

“nere morborum, (tertian intermittents) multo ante accessio-
 “nem caput ægri multâ calidâ aquâ perfundebat, deinde vi-
 “num dabat. Quod quamvis pleraque ejus viri præcepta
 “secutus est Asclepiades, recte tamen præteriit, est enim
 “anceps.” *Celsus, lib. iii. cap. xiv.* This use of the tepid
 or warm affusion is altogether different from what I propose,
 and was not only, as Celsus supposes, a doubtful, but proba-
 bly an injurious practice. For by chilling the body, it would
 dispose it to the accession of the paroxysm. The use of the
 tepid affusion as now proposed, may fairly be considered as a
 new practice.

improvement

improvement derived to her beauty is recorded in a strain of poetry worthy of the subject. (*Odys. lib. viii. l. 362, to 367.*) Ulysses is represented as *laved* by attendant nymphs in the court of Alcinous, and in the island of Circe the whole process of heating the water, and pouring it over the naked hero, is particularly described.* On this occasion also, nymphs administered to him, who after the ablution anointed him all over with oil; a service well calculated in every respect to increase the pleasure, and to diminish the risque of the tepid affusion.

The Romans under their Emperors, carried the system of bathing to a height of luxury and expense which it never reached in Greece or Asia, as the ruins of their magnificent *Balnea* prove to this day; and the affusion of warm water was one of the methods by which they diversified this favourite gratification.†

* *Odys. lib. x. l. 358, &c.* compare this with the bathing of Telemachus in *lib. xvii. l. 85, &c.*

† See *Hieronymi Mercurialis de Arte Gymnast: lib. i. p. 44 and 45.*

CHAP. XI.

Of the internal use of cold water in fever.

BEFORE I conclude the account of my experience of the use of cold water in fever; it will be necessary for me to say something of its effects when swallowed, on the stomach, and through it on the system at large. Among the ancients the internal use of cold water in ardent fevers is recommended by Hippocrates, Galen, Celsus, and most of the celebrated physicians whose works have come down to us: among the moderns, that extraordinary man, Cardanus, wrote a dissertation in its favour, and to pass over a multitude of inferior names, Hoffman, though with some restrictions, recommends it, not in fever only, but in various other diseases*. In our own country it was proposed as an almost universal remedy by Smith, and a treatise has been written

* *Hoffmanni Opera*, vol. i. p. 479.

on it under the title of *Febrifugum Magnum*, by Dr. Hancock. In Spain and in Italy the use of cold water in fevers, obtained in the beginning of this century, a greater and more general reputation than in any of the other countries in Europe, and at one time, seems to have superseded all other diet, as well as medicine. This treatment was celebrated under the title of *Diæta Aquea*, and an account of it may be found in the 36th volume of the Philosophical Transactions, by Dr. Cyrillus, a professor at Naples, to which the reader is referred. Besides the internal use of cold water, he mentions the advantage of laying powdered ice or snow on the bodies of the sick.

Nevertheless, the propriety of giving cold water in fevers has been disputed by men of high character, and particularly by the celebrated Boerhaave. His doctrine, that a lentor in the blood is the cause of fever, led him to insist on the use of warm drink, and the danger of cold; and his commentator Van Swieten, though he allows cold drink in some instances, yet in general argues against it*. These learned theorists prevailed in their day over the voice of nature, and the precepts of Hippocrates and Hoffman.—

* See Boerhaavii *Aphorism.* Sect. 743. with the commentary of Van Swieten.

In the writings of Pringle, Cleghorn, and Lind, we find little or nothing on the subject, though they wrote expressly on fevers; Dr. Cullen mentions cold drink, but gives no opinion on the propriety of its being used, and certainly did not recommend it in practice. He was even doubtful of the extent to which cold air might be admitted*. On the whole it may be asserted, that the use of cold drink in fever is contrary to modern practice, and that where it is occasionally given, it is administered with caution, and rather permitted than enjoined.

It is not however to the doctrine and precepts of Boerhaave alone, that the disuse of cold drink in fevers is to be imputed. The propriety of giving it freely has been at all times controverted, not on the ground of theory only, but from experience of the dangerous, and sometimes suddenly fatal effects of large draughts of cold liquids, various instances of which have been recorded from the earliest periods of medical history. While therefore some physicians have prohibited the use of cold drink in fevers altogether, those who have recommended it from experience of its salutary effects, have introduced various caution

* See his First Lines—cure of fever.

as to its exhibition, founded on certain theories, generally fallacious, on the manner in which its deleterious influence is produced. To detail the various opinions that have prevailed on this curious and important point, would be to add another chapter to the ample records of human errors. That the danger arising from cold drink depended on the great difference between the temperature of the liquid and of the body, and that it is therefore to be prohibited when the heat of the body is very great, is an opinion very generally received by the moderns; and among the ancients, though their doctrines were less erroneous, yet while the means of ascertaining the real heat of the living body, and the changes it undergoes, were unknown, it cannot be expected that they should have arrived at the truth on this important subject.

The effects of cold water as a drink in fevers, I was naturally led to examine by my experience of its effects as an external application. I have made this examination with the thermometer in my hand, and with all the attention in my power; and the following results, which will save the reader the fatigue of reading the particulars of various cases and experiments, seem to me to contain all the information necessary to direct our practice.

1. Cold water is not to be used as a drink in the cold stage of the paroxysm of fever, however urgent the thirst. Taken at such times, it increases the chilliness and torpor of the surface and extremities, and produces a sense of coldness in the stomach, augments the oppression on the præcordia, and renders the pulse more frequent and more feeble. Its effects in all these respects are similar to the affusion of cold water on the skin in the same stage of the paroxysm, as described in pages 18 and 38, though inferior in degree. If the thirst is gratified in the cold stage of the paroxysm, it ought to be with warm liquids.

2. When the hot stage is fairly formed, and the surface is dry and burning, cold water may be drunk with the utmost freedom. Frequent draughts of cold liquids, at this period, are highly grateful; they generally diminish the heat of the surface several degrees, and they lessen the frequency of the pulse. When they are attended with these salutary effects, sensible perspiration and sleep commonly follow.—These effects are similar to those produced by the affusion of cold water on the surface, as already described, but inferior in degree also. Though various cases are on record of the paroxysm of fever being dissolved by cold water, drunk in this stage of the disease, my experience does not furnish me with any instance of
this

this kind*. Indeed since I became acquainted with the extraordinary efficacy of the affusion of cold water on the surface, I have not trusted the solution of the paroxysm to its internal use. I have however employed cold drink when necessary as an auxiliary. Throughout the hot stage of the paroxysm cold water may be safely drunk, and *more freely in proportion as the heat is farther advanced above the natural standard*. It may even be drunk in the beginning of the sweating stage, though more sparingly. Its cautious use at this time will promote the flow of the sensible perspiration, which after it has commenced, seems often to be retarded by a fresh increase of animal heat. A draught of cold water taken under such circumstances will often reduce the heat to the standard at which perspiration flows more freely, and thus bring the paroxysm to a speedier issue.

3. But after the sensible perspiration has be-

* See *Alpinus. Med. Meth. lib. ii. cap. 3.* After reciting the effects of cold drink in diminishing heat and thirst, and exciting profuse perspirations and large discharges of urine, he concludes, *Mirabile est, quomodo tale præsidium, hæc febres expūgnat; nam excretionibus, quas aqua suscitât, hæc febres finiuntur.* According to the same author, this practice was followed by the Egyptians. See *lib. ii. cap. 15.*

come general and profuse, the use of cold drink is strictly to be forbidden*. At this time I have perceived in more than one instance, an inconsiderate draught of cold water, produce a sudden chilliness both on the surface and at the stomach, with great sense of debility, and much oppression and irregularity of respiration. At such times, on applying the thermometer to the surface, the heat has been found suddenly and greatly reduced. The proper remedy is to apply a bladder filled with water, heated from 110° to 120° ; to the scrobiculus cordis, and to administer small and frequent doses of tincture of opium, as recommended by Dr. Rush. By these means the heat is speedily restored.

This effect of cold water used as a drink during profuse perspiration, is precisely analogous to the affusion of it at such times on the surface of the body, a practice known to be of the utmost danger, and enumerated by Hoffman among the causes of sudden death. *Inveniuntur in amplissimo regno naturæ plura quæ brevi tempore nocent ac perimunt, ut aqua frigida corpori sudore diffluentis immoderatus superingesta.* Vol. i. p. 194.

* The pernicious effects of cold drink during profuse perspirations were known to the ancients.—See Celsus, lib. i. cap. 3.

The pernicious effects of cold water applied internally and externally during profuse perspiration, depend on the same causes, namely, that perspiration itself is a cooling process, under which when profuse, the heat of the body, whatever its actual state may be, is sinking; that under such circumstances, we find as a matter of fact, that it parts with its remaining heat more easily; and on the sudden application of cold, that this heat sinks to a degree which disturbs, and sometimes wholly interrupts the actions on which life immediately depends.

Thus then we may safely adopt the same general rules for the use of cold water in fever as a drink, that have already been laid down for its external application. It may be used as a drink, in fevers, at any time, *when there is no sense of chilliness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse sensible perspiration.**

Though we have arrived at these conclusions, the effects of cold water used as a drink have not been inferred from its external application, but made the subject of separate inquiry. Yet, that in the one case, and in the other, they should be

* See p. 17.

similar in kind, though different in degree, will be expected by every one acquainted with the laws of the animal œconomy, and particularly with the sympathy that subsists between the stomach and the surface.

I have only to add, that in our common contagious fever, when I have used the affusion of cold water, I have seldom found it necessary to employ it largely as a drink, and my experience of its effects when drunk in large quantities, has been chiefly confined to those cases, where the fears or prejudices of the patients, or their friends, have prevented our having recourse to the more powerful method of affusion. For however burning the thirst may be, it is speedily abated, and even removed, with very little drink, and often without any, by the successful use of the affusion on the surface. Though the affusion in general suffices in *our* contagious fever, yet where cold water is employed in the dreadful fever of Philadelphia and the West Indies, it is probable, that its internal and external use should be combined; a point that must be determined by the actual heat of the patients, measured by the thermometer, and by their sensation of heat; circumstances of which it is to be regretted that we have as yet no accurate information.

CHAP. XII.

*Of the disease that arises from drinking cold liquids,
or using the cold bath, after severe exercise.*

IT is here natural to inquire how far the fatal effects proceeding from drinking cold water, not in fever, but in cases where the system has been extremely heated by bodily exertions (of which the records of medicine afford so many instances) are to be explained on the principles already laid down. If they are explicable on these principles, we ought to be able to shew, that they have occurred in situations where the system, after having been much heated and enfeebled by severe exertions, is losing its preternatural heat from profuse sweating, and in general also from the cessation of the exertions by which this heat was originally produced. Here two powerful causes combine to cool the body, and if under their operation, a sudden application of cold is made either to the

H

stomach

stomach or the surface, the living power will, we know, resist it faintly, and the fatal consequences be accounted for.

1. In my own experience this sudden death has occurred once only, and that many years ago. It was in the case of a young man who had been engaged a long time in a most severe match at fives. After it was over, he sat down on the ground, panting for breath, and covered with profuse perspiration. In this state he called to a servant to bring him a pitcher of cold water just drawn from a pump in sight. He held it in his hand for some minutes, but put it to his head as soon as he had recovered his breath, and drank a large quantity at once. He laid his hand on his stomach, and bent forwards; his countenance became pale, his breath laborious, and in a few minutes he expired. Various methods were employed to restore him, but in vain.

2. The following case resembles very exactly that just given. *Blasius, Senensis, familiaris noster et condiscipulus, dum longiusculo tempore sub ardentissimo sole pilæ lusu incaluisset, nec sudore adhuc aut fatigatione remissis, in subterraneum locum ubi vinaria erat cellula, descendisset, frigidissimi vini calicem hausit; quo epoto, statim defecit.* Benevent. cap. 17. De abditis.

3. *Elegans*

3. *Elegans & optimæ staturæ juvenis Romanus, cum pilâ luderet, et sudore respersus, ac totus madidus, & fatigatus ad puteum, pro siti ar-cendâ venisset, exhaustâ frigidâ recens per calda-rium extractâ, illico in terram cecidit & obiit.*

4. *Alterum novimus ex iis, qui in campo ne-gotiantur, qui quum non minus corporis totius adapertis poris madidus domum reverteretur, cy-athum frigidioris aquæ ebibit, et mortuus fuit.*
Anat. Lusit. curat. med. cent. 2. curat. 62.

5. Forrestus relates, that in the year 1544, Valerius Cordus, a young man of great learning and talents, went during the heat of the dog-days, to collect plants among the Florentine mountains. Exhausted with fatigue and thirst, he incautiously drank of a cold spring which issued out of one of the hills, and was immediately seized with a fever, of which he died; but the symptoms of which he has not recorded. *P. Forestus, lib. i. Scholio ad obs, 13.*

6. Scaliger relates the case of a reaper, who, stooping down to drink at a fountain after severe labour, instantly expired. *Scaliger de Subt. ad Cardan. exer. 13.*

7. In Heister's observations, a case is related of
H 2 a young

a young man, who, about the Christmas season, had been playing and dancing at a Mill with some young women, and had eaten greedily of some hot buttered cake. After this, being extremely thirsty, he took a large draught of some cold water mixed with snow. An inflammation in the stomach followed, terminating in mortification, of which he died. *Heister's Med. &c. Observations, translated by Wiseman, p. 17.*

8. *Villanum quendam nobis familiarem novimus, qui messis tempore, anno 1597, exhaustus viribus, et totus sitibundus præ nimio solis ardore, domum rediens, cum in magna quantitate, ad sitim explendam, gelidam (aquam) bibisset, exanimatus mox juxta puteum cecidit, ac intra tres horas animam expiravit. Georgius Grasseccius in Theatro Anatonico.*

These relations are chiefly taken from the collection of Schenck—they might be greatly enlarged, and I have collected a number of similar cases; which it were a tedious and an useless task to detail. In all of the cases which I have consulted, as well as in those I have related, three circumstances are either expressed or may be clearly inferred—1. The body had been previously heated beyond the temperature of health, by exercise carried to fatigue. 2. To this violent exertion

exertion a state of rest had succeeded. 3. A profuse perspiration had taken place. So far our reasoning is supported; but as these points are of the utmost consequence in explaining the operation of cold on the human body, and as direct experiments are attended with extreme hazard, the reader will excuse me, if I attempt to illustrate them by such evidence as history may incidentally afford.

1. In Quintus Curtius, (*lib. vii. cap. 5.*) an account is given of the march of the army of Alexander the Great in pursuit of Bessus, through the country of the Sogdiani, which is represented as destitute of water, sterile, and covered with scorching sands. The intolerable heat, fatigue, and thirst of the soldiers, in their march through this burning desert, are described with all the florid eloquence of the historian. At length, fainting under their toils, they reached the banks of the river Oxus, where by indulging large draughts of the stream, Alexander lost a greater number of his troops than in any of his battles. *Sed qui intemperantius hauserant intercluso spiritu extincti sunt; multoque major horum numerus fuit, quam ullo amiserat prælio*.*

2. A similar

* The whole particulars of this march as described by Quintus Curtius, are very interesting. The desert, which

2. A similar story is related by Appian—*Appianus Alexandrinus de bellis civilibus, lib. v. tradidit Cornificianos milites a Pompeianis pugna fatigatos & æstuentes fontanam aquam avidè bibentes,*

contained not a drop of water, was four hundred Stadia across—(*per quadringenta stadia ne modicus quidem humor existit*) that is, upwards of forty-six English miles. They began their journey in the night, directing their course by the stars, and for some time their march was tolerable, being refreshed by the dews of the night, and the coolness of the dawn; but when the sun rose, the heat became troublesome, and as the day proceeded, most oppressive; it was equally painful to stand still or proceed. After a day of dreadful fatigue, the vanguard of the army, and Alexander himself, reached the Oxus towards evening; and such as were themselves refreshed, were employed in carrying water back to the fainting troops behind. As they arrived in succession on the banks of the river, it may easily be supposed that they drank without moderation, and hence the destruction that ensued. It was on this occasion that Alexander displayed his magnanimity, in refusing the cup of water brought to him as he advanced, because it was not sufficient both for him and his companions; and that he gave a proof of his genius, by ordering fires to be kindled on the high banks of the Oxus, not merely to direct his way-worn soldiers through the darkness, but to animate their fainting exertions by a prospect of the end of their toils. Plutarch alludes to this story, but does not relate it fully. I fear it is not to be found in Arrian. A similar distress is mentioned by him (*lib. vi. p. 425*) to have occurred in Alexander's march through the deserts of the country of the Gedrosi, who inhabited the southern part of the Persian empire, on the shores of the Indian ocean.

bentes, ex iis plurimōs emortuos. Marcel. Donat. lib. iv. cap. 6. Hist. med. mirab.

3. A disaster of the same kind is recorded to have occurred to the Christian army in the holy wars. *Gulielmus Tyrius, lib. iii. c. 16. scribit, Christianum agmen Pisidiam, ingressum, regionem ærentem & in aquosam, tandem invento fluvio, avidè bibisse; quod quidem, qui largius aquam frigidam ingurgitaverunt, sitis discrimen exadentes, mortem in aquarum opulentiâ reperére.*

These historical relations support very fully the doctrine I have already laid down. The cautious reasoner may not, on a hasty consideration, be inclined to rest with much confidence on this sort of evidence; but on reflection he will see that it is entitled to considerable authority; because the facts are in their nature not liable to be mistaken, and because they are not likely to be misrepresented. It is also entitled to much weight, because it is not given by these historians in support of any particular doctrine; and because the experiment having been made on such numbers of persons at once, it acquires an authority hardly to be ascribed to solitary cases, however accurately detailed. These considerations induce me to lay much more stress on evidence of this kind, than on the precepts respecting the effects of cold drink to be found

found in medical authors, ancient or modern. Nevertheless I have looked into the greater part of the ancients on this point, (for among the moderns there is not much on the subject) and have found nothing, that fully considered, invalidates the conclusions I have laid down.

Of the ancient physicians, the most copious on the use of water, in all its forms, is Galen. He not only used cold drink, but immersion in the cold bath, in burning fevers, with extraordinary success. His relations appear to me, in general tedious and obscure, but not destitute of truth; and the weariness of perusing him, is occasionally relieved by the pleasure of rescuing a fact that was buried under masses of false theory. The reader who would consult him on this subject may use the references below*.

In the first volume of *Medical Inquiries and Observations*, published by Dr. Rush of Philadelphia, 1789, an account is given of the "Disorder occasioned by drinking cold water in warm
" weather,"

* Vol. i. 23. B.

Vol. ii. 78. C.

Vol. vii. 70. A. and forwards throughout the volume. I quote from the Latin edition in folio, published at Venice, 1656.

“weather,” which frequently occurs there. “Three
 “circumstances,” he observes, “generally concur
 “to produce disease or death from drinking cold
 “water. 1. The patient is extremely warm. 2.
 “The water is extremely cold. And 3. A large
 “quantity of it is suddenly taken into the body.
 “The danger from drinking cold water is always
 “in proportion to the degrees of combination
 “which occur in the three circumstances that have
 “been mentioned.” *p.* 151. Dr. Rush goes on to
 state the symptoms of this disease, which are, I ap-
 prehend, given with accuracy. His method of
 cure my experience neither authorises me to con-
 firm nor oppose. “I know but one certain remedy
 “for this disease, and that is *liquid laudanum*.
 “The doses of it, as in other cases of spasm, should
 “be proportioned to the violence of the disease.
 “From a tea-spoonful, to near a table-spoonful,
 “has been given in some instances before relief has
 “been obtained. Where the powers of life ap-
 “pear to be suddenly suspended, the same reme-
 “dies should be used which have been so success-
 “fully employed in recovering persons supposed to
 “be dead from drowning.” To this I would add
 the application of a bladder filled with water,
 heated to 110° or 115° of Fah°. to the pit of the
 stomach, from which I have seen powerful effects
 in restoring the vital heat. But while I do not
 dissent.

dissent from Dr. Rush's practice, I cannot subscribe to his notion of the causes of this disease, or to the method of prevention founded on this notion.

Dr. Rush seems to entertain the popular opinion on this subject: *the body is extremely warm; the water extremely cold, and a large quantity is introduced suddenly.* He apprehends the danger to arise from the great difference between the temperature of the body, and of the water taken in. As a means of prevention he therefore proposes to such as cannot be restrained from drinking cold water when preternaturally heated—

1. To grasp the vessel out of which they are about to drink for a minute or longer with both hands, that a portion of heat may be abstracted from the body and imparted to the cold liquor.
2. If they are not furnished with a vessel to drink out of, but obliged to drink at a pump or a spring, always to wash their hands and face previously to drinking, with a little of the cold water.

“By receiving,” says he, “the shock of the water first on those parts of the body, a portion of its heat is conveyed away, and the vital parts are defended from the action of the cold.”

The fact however is in my mind perfectly established,

blished, that there is no situation in which the application of cold to the body, whether to the surface or the stomach, is so safe, or in general so salutary, as when the heat of the body, from whatever cause, is preternaturally great, provided that the body is not already in a state in which it is rapidly parting with this heat, and no disease has taken place in the general sensibility, or in the structure of any of the parts; and that where the body is preternaturally heated, the degree to which cold water may be drunk, may be always decided by the steadiness of the sensation of heat, and the tenacity with which the preternatural heat is actually retained. Thus, in continued fevers it may be drunk to a greater extent, than in the hot stage of intermittents, because the heat is more firmly retained; the profuse perspiration not being at hand, by which the febrile heat of intermittents is carried off. The ancients who gave cold drink largely in continued fevers, were doubtful of its use in intermittents.* It may however be given (as

* See Sennertus, lib. ii. cap. ix. p. 54. Itaque Græci auctores jubent in statu, cum febris acuta, sitis, inquietudo, cordis & arteriarum pulsatio est vehementissima, ægerque avidissime eam expetit, aquam frigidam copiose exhibere.—

* * *

In intermittentibus febribus vero aquæ frigidaë potus nunquam convenit.

(as I have already stated) with great safety in intermittents, provided it be taken in the time that intervenes after the hot stage of the paroxysm is fairly established, and before the sweat that follows it, has become general and profuse. As however it is only in that interval that it can be given in intermittents with advantage or safety, we can easily understand, that the ill effects arising from its being accidentally drunk in the cold, or the sweating stage of the paroxysm, (in both of which the thirst often demands liquids) may have produced the doubts which some have expressed in regard to its use in intermittents, and the interdiction which in such cases others have pronounced against it. We may explain also from the same considerations, why in the accounts that have been handed down to us of injurious effects from the use of cold drink in fevers, the greater part of the cases have been intermittents.

The instances however that are recorded of the fatal effects of large draughts of cold liquids, have more frequently occurred after severe exercise and fatigue, than even in intermittent fever. The cause of this is obvious—the heat preternaturally accumulated by exercise, is held with less tenacity than even the heat in intermittents. It is dissipated by the perspirations that exercise occasions, and is speedily lost, when to profuse perspiration

spiration is added a state of rest. It is then that a large draught of cold liquid is especially dangerous. But while the preternatural heat is sustained by continued exertion, cold liquids may be taken in moderate quantities without producing any injurious effects. They may even, I apprehend, be drunk copiously without producing suddenly the fatal effects already described—but in copious draughts, they are found oppressive to the stomach during exercise, and excite languor, nausea, and sometimes vomiting, as I have had occasion to observe. In the narrative already mentioned of the march of Alexander's army through the desert country of the Sogdiani, it is related by the historian, that a few of the soldiers, by the advice of the natives, had provided themselves with water, of which under their burning thirst they drank immoderately. The consequence was, that they became heavy, feeble, and unable to support their arms, and this state of oppression was succeeded by severe vomiting. *Graves deinde avidè hausto humore, non sustinere arma; non ingredi poterant; et feliciores videbantur, quos aqua defecerat, quum ipsi sine modo infusam vomitu cogarentur egerere.* Q. Curtius, lib. vii. cap. 5. The water thus wasted, or worse than wasted, might have been used to advantage in wetting, from time to time, the garment next the skin. Thus the oppression of its weight on the stomach

stomach would have been prevented, and the surface of the body being kept cool by constant evaporation, the heat of the system would have been moderated, and the thirst alleviated*.

If this account of the circumstances under which cold drink after severe exercise proves injurious, be just, the directions of Dr. Rush to those who *will* drink in such circumstances, are founded on error. By abstracting a part of the preternatural heat of the body before drinking, the danger is not diminished, but greatly increased. This enlightened physician will excuse these observations, drawn from me by a consideration of the importance of the subject, but accompanied by sentiments of sincere esteem and respect.

If

* Postilions understand the difference between giving their horses cold water to drink, during exercise, and after the exercise has ceased. When in their power, they always water their horses two or three miles before the end of their journey. My friend, Mr. Charles Aikin, assures me, that during his tour on foot through Wales, in company with his brother, of which Mr. Arthur Aikin has given so interesting and useful an account, they drank of the pure streams, as they descended from the mountains, without reserve, during the fervor of the day, taking care however never to rest after drinking.

If the effects of cold water used internally under severe exercise are not entirely analogous to the effects produced by its affusion on the skin;—the difference will be easily understood, by those who consider, that where a quantity of water is swallowed, besides the influence of the cold, the stomach sustains a load, from the weight and the bulk of the liquid, particularly oppressive under the constant action and agitation of the voluntary muscles, from which the surface, moistened with water, is entirely free; and on the other hand, that the evaporation from the surface, promoted by the immediate access of the external air, must operate more directly in cooling the body, and particularly in counteracting the burning rays of the sun, than water taken into the stomach. With these exceptions the operation of cold liquids on the stomach and on the surface of the body are analogous in the case of preternatural heat produced by bodily exertion, as in all other cases of preternatural heat. As it is safe to drink cold water in proportion as the heat from exercise is great and steady, so also is it safe, according to this ratio, to pour it on the surface, or to immerse the body in the cold bath.

In the earlier stages of exercise, before profuse perspiration has dissipated the heat, and fatigue debilitated the living power, nothing is more safe,
according

according to my experience, than the cold bath. This is so true, that I have for some years constantly directed infirm persons to use such a degree of exercise before immersion, as may produce some increased action of the vascular system, with some increase of heat; and thus secure a force of re-action under the shock, which otherwise might not always take place. The popular opinion, that it is safest to go perfectly cool into the water, is founded on erroneous notions, and is sometimes productive of injurious consequences. Thus, persons heated and beginning to sweat, often think it necessary to wait on the edge of the bath until they are perfectly cooled; and then, plunging into the water, feel a sudden chilliness that is alarming and dangerous. In such cases the injury is generally imputed to going into the water too warm, whereas in truth it arises from going in too cold.*

But

* Dr. A. Munro Drummond, in his inaugural dissertation, "*De Febris Arcendis*," the only specimen left of his admirable talents, speaking of the effects of the cold bath as a preventive against the action of contagion, observes, "Nec frigida protinus fugienda vel calida temperanda, quamvis cutis pallida aliquantisper siat vel leviter aliquis inhorruerit, Si exercitatio antecedit quæ citra lassitudinem & sudorem est, hæc facile evitari posse experiendo didici: et fere, si nil aliud obstat, quo ante quisque plus incalueret quam aquam intraverit, eo calidior emerget."

But though it be perfectly safe to go into the cold bath in the earlier stages of exercise, nothing is more dangerous than this practice after exercise has produced profuse sweating and terminated in languor and fatigue. Because, as has already been repeated more than once, in such circumstances, the heat is not only sinking rapidly, but the system parts more easily with the portion that remains.

This account of the operation of the cold bath, will explain some circumstances very generally mentioned by writers on the effects of cold on the human body, and hitherto not properly accounted for. That the Roman youth, in the heat of their exercise in the *Campus Martius*, frequently plunged into the Tyber, is a fact universally known; they found in this practice a high enjoyment, and they believed it conducive to health, and more especially to sleep. On the other hand, various relations may be cited of the injurious effects of an apparently similar practice; the most remarkable of which is the case of Alexander the Great, when covered with dust and sweat, he threw himself into the Cydnus, and was seized with a disease, of which he nearly perished; one of the best authenticated facts in ancient history.

In the dissertation *De febris arcendis*, by Dr.
I A. Munro

A. Munro Drummond, these facts are brought together in the following words. *Alexander, quondam diei fervidissimo tempore, liquore fluminis invitatus, rix dum in Cydnum amnem descenderat, cum gravi inde morbo implicitus est. Romana juvenus, post quotidianas in Campo Martio exercitationes, pulvere simul & sudore perfusa, Tyberi impune lassitudinem cursus nandi labore deposuit. Mali nimirum aduetudine duramur in his sicut in cæteris rebus omnibus.* *Thes. Med.* vol. iii. p. 154. Doubtless the influence of habit has a considerable share in regulating the effects of cold on the human body; but the circumstances just mentioned, seem capable of an explanation on other principles.

On the *Campus Martius*, the exercises of the Roman youth were carried on with all the vehemence of emulation. Swimming formed a part of these exercises*, and generally terminated the foot-race. The youthful candidates in this exercise directed their course towards the banks of the

* See *Horace*, lib. i. ode 8. Where the poet, after questioning Lydia why her lover neglects his accustomed exercises on the *Campus Martius*, among other particulars asks,

Cur timet fluvium Tiberim tangere?

See *Vigotius* lib. i. cap. x.

See also *Hieron. Mercur.* lib. iii. cap. 14.

river, and plunged headlong into the stream. Sometimes the contention did not terminate till they had swum across the river twice. Hence it will easily be seen, that they were accustomed to immerse themselves in water in the very fervor of their exertions, when the heat was preternaturally great; and not after the body was cooled by profuse perspirations, or exhausted by long continued fatigue. In this situation the practice was safe; without taking into consideration, that the persons following it, were in the flower of life, fortified by early habits, and partly defended from the shock of immersion by the inunctions which seem to have been generally used among the Romans, before the cold*, and after the hot bath;

I 2

and

* See Horace, *Satyr: lib. ii. sat. 1.*

—————*Ter uncti*

Transnanto Tiberim, somno quibus est opus alto.

The subject of the ancient unguents is treated of by Hieronymus Mercurialis, *de arte Gymnastica, lib. i. cap. x.*

The same author, (*lib. iii. cap. 14.*) speaking of swimming, so much in estimation among the ancients, observes, *quo pacto vero similis exercitatio perageretur, tanquam res vulgatissima silentio præterita fuit fere ab omnibus, uno excepto Antyllo, qui nataturos prius moderatè unxisse, et frictione corpora præcalfecisse, deinde e vestigio in aquam se projicere debere voluit, quasi omnes sanitatis gratia natantes eas regulas observarent.*

This

and which were particularly employed by the *athletæ* of Greece and Rome in all their exercises. It was the more safe, because the stream of the *yellow Tiber*, being comparatively scanty and slow, its waters speedily received the influence of the sun, and acquired the temperature of the atmosphere. Where the air and the water are of the same temperature, the rarer element prepares the body for the contact of that which is more dense.

The circumstances under which Alexander plunged into the Cydnus, were different in many essential points. He had marched at the head of his troops to seize a pass in Mount Taurus, necessary to facilitate his passage into Cilicia. After having secured his object, he descended from his elevated station, through a road difficult and full of defiles, to the city of Tarsus, which was situated at the bottom of the mountains. His whole march, which probably continued several days, was attended by extraordinary exertion, not from the nature of the country only, but also from the pressure of circumstances.

This method of previously anointing the body might be recommended to our modern swimmers, not only as defending them from the shock of immersion, but as enabling them to glide through the water with less effort.

In

In ascending the heights, he had to hasten forwards, lest the enemy should pre-occupy the pass by which Cilicia must be entered. In descending from the immense elevation he had reached, he was stimulated by the desire of saving the city of Tarsus, the capital of the province, which the Persians threatened to consume by fire. In both of these enterprizes Alexander was successful. It was at the close of this extraordinary march, according to the testimony of all the historians, that the conqueror, advancing into Tarsus at the head of his troops, covered with dust and sweat, and exhausted with long continued toil, stripped himself in the sight of his army, and plunged into the pure and cold waters of the Cydnus, which ran through the city. The symptoms that followed, are described with sufficient minuteness and precision. *Vixque ingressi subito horrore artus rigere cœperunt: pallor deinde suffusus est, et totum propemodum corpus vitalis calor reliquet. Exspiranti similem ministri manu excipiunt, nec satis compotem mentis in tabernaculum deferunt. Q. Curtii, lib. iii. cap. v.* From the length and difficulty of the march, it is natural to suppose, that Alexander must have been cooled, as well as debilitated, by excessive perspiration and fatigue, and under such circumstances, immersion in the cold and rapid Cydnus, was followed by the consequences which

we

we should expect from the principles already laid down*.

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* If an objection be urged against our laying much weight on a relation taken from a period of history so remote, I reply, that I am far from resting any part of my conclusions upon it, having the firmer foundation of actual observation. But as this adventure of Alexander is mentioned by almost every writer on the effects of cold on the living system, to exemplify the danger of its being applied when the body is preternaturally heated, it seemed proper to shew, that it is capable of a more just and natural explanation.

In regard however to the fact itself, of Alexander having suffered from bathing in the Cydnus, it might be easily shewn, that we cannot reject it without resisting entirely the weight of historical evidence. 1. This exploit was performed before thousands of witnesses, by some of whom it is recorded, and it is mentioned by all the historians. 2. The circumstances that preceded it, and the consequences that followed it, are also recorded with great minuteness, and form a natural chain of events. 3. The act itself of stripping before his army and springing into the Cydnus, may have an air of improbability; but is not so inconsistent with Greek, as with modern manners; and however unlike the dignity of great commanders in our days, is perfectly characteristic of Alexander—of that ardent and daring temper, and of that buoyant imagination, which were equally regardless of danger and decorum; a temperament, that joined to extraordinary talents, attached his soldiers to him in so singular a degree, and which in the æra of the world in which he lived, was calculated to carry him to the astonishing elevation which he attained. 4. The exploit itself was of a nature not likely to be feigned; and the issue of it had nothing marvellous—there

was

In his essay on swimming, Franklin makes the following observation : “ During the great heats
“ of

was nothing in any part of it, by which the son of Ammon, or his followers, could be in any way gratified.

The effects of Alexander's bathing in the Cydnus, gave a celebrity to this river which it otherwise would not have acquired. I have called it cold and rapid, meaning to speak of it comparatively with the Tiber. The Cydnus is one of the principal streams that descend from Mount Taurus, (*Strab.* xiv. p. 462.) at the bottom of which stood the city of Tarsus. From the great elevation of the mountain, the Cydnus must in this situation be rapid and cold; and colder because rapid. From the same cause it would be easy to shew (if it were at all to the purpose) that the transparency ascribed to its waters was probably ascribed justly. Quintus Curtius imputes its coldness to the rays of the sun being excluded by the shadiness of its banks; the more accurate and philosophic Arrian, mentions as the cause, the loftiness of the mountains whence its fountains descended. Quintus Curtius speaks of it as gliding through a gentle descent, (*leni tractu e fontibus labens*) and Tibullus of its silent and placid stream.

*At te, Cydne, canam, tacitis qui leniter undis,
Ceruleus placidis per vada serpis aquis.*

Strabo, on the other hand, (*lib.* xiv. 392.) mentions its current as most rapid, and this to some of the scholiasts seems a contradiction. It is not so in reality. If the declivity was uniform, the current might be at once swift and silent. The river Moffat (the source of the Annan) descends from the mountain of Hartfell, by a declination, which in three thousand

“ of summer there is no danger in bathing, how-
 “ ever warm we may be, in rivers which have
 “ been thoroughly warmed by the sun. But to
 “ throw ourselves into cold spring water, when
 “ the body has been heated by exercise in the
 “ sun, is an imprudence which may prove fatal.
 “ I once knew an instance of four young men,
 “ who having worked at harvest in the heat of
 “ the day, with a view of refreshing themselves,
 “ plunged into a spring of cold water; two died
 “ on the spot, a third the next morning, and the
 “ fourth recovered with great difficulty. A co-
 “ pious draught of cold water in the same cir-
 “ cumstances is often attended with the same
 “ effect in North America.” The authority of
 the American Bacon is of great weight in medi-
 cine, as in every other branch of science that he
 touches; and particularly in what respects im-
 mersion in water; for doubtless he spent more

sand yards, amounts to two hundred and thirty feet, with
 such uniformity, that in the course of some miles there is
 hardly a single ripple. To finish a digression by far too long,
 but which may relieve the dryness of a professional subject,
 it may be observed, that our account of the illness of Alex-
 ander, receives confirmation from a difference among the
 historians respecting its causes; some imputing it to his hav-
 ing bathed in the Cydnus, and others to his previous fatigue:
 at the distance of two thousand two hundred years, it may be
 clearly determined, that both circumstances combined in pro-
 ducing his disease.

time

time in this element than any philosopher of modern days. It may however be easily supposed, that he adopted the commonly received opinion; that the injury arose from the persons in question going in *when hot*, instead of from going in *when cooling, after having been heated*; to which latter circumstance, it can hardly be doubted, that the fatal accident he relates, was to be imputed. It is not however to be denied, that the difference between the temperature of spring and river water, at certain seasons of the year, will make a great difference (in any circumstances of the body) in the consequences of immersion. Springs which issue copiously from the earth, are, as we know, both from reasoning and observation, nearly of the temperature of the earth at some distance under the surface; and this temperature, when unaffected by chemical changes, is probably (for actual experiment is not yet sufficiently extensive to affirm the proposition universally) of the mean heat of the atmosphere, taking the year round, under which it lies*. On the other hand, rivers, even of the largest size, acquire at a certain distance from their source, the temperature of the earth, and rise towards the end of summer, to nearly the highest degree of the summer

* See Dr. John Hunter's valuable paper on this subject, in the Philosophical Transactions for 1788, p. 53.

heat*. In countries where the temperature of the seasons has little variation, the difference between the heat of the rivers and springs will be very inconsiderable. That this is the case in Jamaica, we know from the experiments of Dr. Hunter; but in Pensylvania, to which it is probable that Dr. Franklin's observations more particularly refer, this difference, at certain seasons of the year, must be very great, the difference of the temperature of the seasons being so very considerable. The rivers of Pensylvania in the latter end of summer, are probably nearly as warm as the Buxton bath, or from 78° to 82° ; and in the Buxton bath there is reason to believe, that even after considerable fatigue, the chill of immersion is too inconsiderable to produce much hazard.

* Thus the Mersey, at Liverpool, in the middle of the tide, is in the month of August from 65° to 69° . On the 20th of August 1795, it rose in the middle of the tide to 70° and under the rocks at High Park, the mercury stood on the 21st, at high water, at 72° .—In the air, at the same time, it was at 77° .—The weather had been uncommonly warm for fourteen days before, and 72° was probably the average heat of that fortnight. It seems to be this average that the tide of the Mersey acquires. Observations made in the middle of our different æstuaries every week, would give the actual and the comparative heat of the seasons (on the coast at least) with much more accuracy than observations in the atmosphere,

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The springs of Pensylvania, and more especially the deep sunk wells, if they retain the subterranean temperature (the medium heat of the seasons) will probably be in the heat of summer and autumn, twenty or twenty-five degrees cooler than the rivers and the atmosphere*; a difference capable of producing, in the situation of the body, which renders the application of cold dangerous, the most serious effects. It is doubtless owing to the great comparative coldness of their springs and wells, that fatal accidents from drinking cold water, and bathing in cold springs, are so frequent in Pensylvania. In the East or West Indies, where water is seldom to be procured below 77°, such accidents, if they ever occur, are very rare†.

I shall

* The wells at New York are from 54° to 56°, according to Dr. Routh. See *Philosophical Transactions* for 1788, p. 61.

† 1803. In an abridgment of this volume, said to have been made by Mr. Vaughan, formerly member of the British House of Commons, and published in North America by Peter Eddes of Augusta, in the district of Maine, the ingenious abridger, who frequently introduces valuable remarks of his own, observes on this passage as follows. "A number of other seemingly contradictory, and yet authentic relations, receive here also a satisfactory solution. Hence we may assure ourselves, that if the waters of the Mississippi never injure those who drink them in summer, whatever be their state as to perspiration or fatigue, it is not owing
" to

I shall close my remarks on the circumstances which render the cold bath safe or hazardous after exercise, with the following narrative:—

On the first of September, 1778, two students of medicine at Edinburgh set out on foot on a journey, a considerable part of which lay along one of the rivers of Scotland. They started by sun-rise, and proceeded with alacrity in the cool of the morning. At the end of eight miles, they breakfasted, rested for an hour, and then resumed their journey. The day grew warm as it advanced, and after a march of eight miles more, they arrived heated, but not fatigued, on the banks of the river above-mentioned, about eleven in the forenoon.—Urged by the fervor of the day,

“to the *quality* of these waters, but to their *warmth*, in consequence of their long exposure to the sun.” There is no doubt of the justice of this observation. The body of water which descends the bed of the Mississippi, immense as it is, running for a thousand miles and upwards, in a channel, not perhaps at its upper end one hundred yards above the surface of the sea, and in the whole of its course exposed to the rays of the sun, must acquire the temperature of the atmosphere; exhibiting however in the middle of its stream rather the average heat of the atmosphere for the three or four preceding days, than at the moment of observation. The same may be said of the waters of the Ganges, of the Nile, and of other great rivers.

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and tempted by the beauty of the stream, they slipped instantly, and threw themselves into the river.—The utmost refreshment followed, and when they retired to the neighbouring inn, this was succeeded by a disposition to sleep, which they indulged. In the afternoon they proceeded, and traveling sixteen miles farther at a single stretch, arrived at the inn where they were to sleep, a little after sun set.—The afternoon had been warm, and they sweated profusely ; but the evening was temperate and rather cool. They had travelled for some miles slowly, and arrived at the end of their journey, stiffened and wearied by their exercise.

The refreshment which they had experienced the morning from bathing, induced however none of them to repeat the experiment, and he jumped perfectly cool into the same river, expecting to relax his limbs in the water, and afterwards to enjoy profound sleep. The consequences were very different. The Tweed, which was so refreshing in the morning, now felt extremely cold ; and he left the water hastily. No genial glow succeeded, but a feverish chill remained for some time, with small frequent pulse, and flying pains over the body. Warm liquids, and friction brought on at length considerable heat, and towards morning perspiration and sleep followed.

next

Next day about noon they proceeded on foot, but the traveller who had bathed was extremely feeble; and though they had to perform a journey of a single stage only, as some part of it was difficult and mountainous, he was obliged to take the assistance of a carriage which overtook them on the road. It was several days before he recovered his usual vigour. This relation will not I hope be deemed of the less authority, because it is given by the person who suffered by his imprudence.* It is unnecessary to point out the application of these incidents to the doctrines already laid down.

It seems to be a general truth, that from whatever cause the heat of the body is increased, in proportion to this increase (provided no local disease has occurred) is the safety with which cold may be applied. In the celebrated experiment

* Those who know the road from Edinburgh to Moray will be at no loss to mark the different stages of this adventure.

I may here observe, that illnesses similar to the above occur frequently, from the imprudence of the country people, (the Boltoners, as they are called) who make an annual visit to the river below Liverpool, for the purpose of bathing. In such cases, I have found the disease that followed, was inflammatory, as might be supposed, but rather a feverish debility, as in the instance detailed.

ments of Dr. Fordyce, Sir Charls Blagden, and others, of which an account is given in the Transactions of the Royal Society, *vol. lxx. p. 111, and 484*, it is repeatedly mentioned, that these gentlemen passed from a room heated to 200°. and upwards, into the cold air, with perfect safety. "During the whole day," says Sir Charles Blagden, "we passed out of the heated room," (where the temperature of the air seems to have been 240°, and sometimes 260,) "after every experiment, immediately into the cold air, without any precaution; after exposing our naked bodies to the heat, and sweating most violently, we instantly went into a cold room, and staid there, even some minutes, before we began to dress; yet no one received the least injury." *Ibid. p. 494.*

The freedom from injury, and even from inconvenience, which these gentlemen experienced, depended on the increased heat of the body, and the increased action of the arterial system.—Had they continued exposed naked to the cold air till the heat sunk as low as its natural standard, and the heart and arteries subsided into their usual state of action, their situation would have been very hazardous.

In similar experiments, repeated at Liverpool,
of

of which a detail is given by Dr. Dobson, in the same volume of the Philosophical Transactions *p.* 463, the gentleman engaged in them passed from the heated room into the cold air, with equal impunity. My friend Mr. Park assures me, that after remaining some time in the stove, where the heat was as high as 202° , he went into the external air without a great coat, or any other than his usual clothing, during a hard frost, and perceived neither injury nor inconvenience.

In many manufactures, the persons employed are exposed to extraordinary degrees of heat, particularly in the manufacture of glass. Such persons, guided by nature, often endeavour to obtain relief, by exposing themselves to degrees of cold, which on the commonly received opinions, would in their situation appear extremely dangerous. Thus at Glasgow, in the spring of the year 1780, I learnt, that it was common for the workmen in the glass manufactory, after enduring for some time the consuming heat of their furnaces, to plunge into the Clyde; a practice which they found in no respect injurious.

A great variety of the phenomena respecting the influence of cold on the living body, receive an easy explanation by attending to the principle

already mentioned. Thus, after the heat of the body is increased two or three degrees in the hot bath, it is not only safe, but refreshing, to plunge into the cold bath, as I have repeatedly experienced. A practice of this kind prevails, as is well known, in Russia, where it is common; after remaining some time in the hot bath, to roll naked in the snow, and return to the warm bath as before. The Russian hot baths vary from $106\frac{1}{2}^{\circ}$ to 116° of Fah^t.* and I find by experiment, that the actual heat of the living body is increased in the hot bath, unless under particular circumstances afterwards to be explained, when the temperature is no higher than 100° . We may therefore safely infer, that in making these singular transitions, the heat of the Russian is in the first instance increased beyond the natural standard; and it is to this increase that the safety, as well as the agreeableness of the practice, is to be attributed; and from all these facts we may conclude, that, where the actual heat is considerably increased,

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* See *Dissert. physica experimentalis de calore Animalium*, by J. A. Braun, in the *Nov. Comment. Academiæ Scientiarum Imperialis Petropolitanae*, vol. xiii. The Abbé Chappe D'Auteroche says the Russian baths are as high as 60° of Reaumer, (160° of Fahrenheit) *Philosophical Transactions*, vol. lxxv. p. 112; but he speaks of vapour baths.

and the body is not weakened by fatigue, even the presence of profuse sensible perspiration will not render exposure to a certain degree of cold dangerous.

It is in this way that we must account for the safety of a practice, which prevailed at Rome under the first of the emperors. After the overthrow of the republic, the Romans consoled themselves for the loss of their freedom, by a more unbounded indulgence than ever in those sensual gratifications which had led to their fall. Of these, the pleasures of the bath formed a distinguished part; they sought every means of heightening and diversifying them, and connected them with other epicurean enjoyments. The mild and tepid immersion of the Greeks no longer satisfied them; they heated their baths to the utmost pitch of endurance; and, as they rose, reeking from their surface, vessels full of cold water were dashed over their naked bodies, as a high gratification in itself, and a means of stimulating the senses to gratifications still higher. Such practices could not however be continued without injury. In the bagnios of *Imperial* Rome, the last of the Romans, the followers of Zeno and Cato, were melted down into slaves; the powers of their minds became enfeebled, the vigour of their frames

frames decayed, and they lost for ever the bold impressions of freedom and of virtue*.

It was supposed by Dr. Cullen, who paid
K 2 much

* This subject might lead to digressions, which my limited plan does not admit. I shall content myself with a few observations. Though private baths were in use in Rome from the earliest periods of their history, yet the system of public hot baths did not, it is understood, commence till the days of Augustus. According to Dion, Mæcenæ introduced it.—Agrippa followed his example—It was soon carried to an astonishing height, and the construction of baths where the people might be accommodated *gratis*, was an established and a successful method of gaining their affections. The extraordinary expense and magnificence of those structures are well known—the remains of the baths of Caracalla and Dioclesian, testify their former grandeur in our own days. According to Fabricius, there were 856 public baths at Rome, and some of these were large enough to contain at once 1800 persons. The *rage* for hot bathing in Rome, under the first Emperors, exceeded all bounds.

In regard to the exact temperature of their baths, as the thermometer was not known to the ancients, we cannot speak precisely. We have however the authority of Seneca and Plutarch for saying, that the bath of the Greeks was of a moderate temperature; and, as Alexander found refreshment in the bath, in the burning fever of which he died, it probably did not exceed 93°, or at most 96°, of Fahrenheit. But at Rome, in the days of Seneca, the hottest baths were most in estimation, and those of Nero seem to have exceeded

much attention to the operation of temperature on the living system, that in all sudden changes, from a higher to a lower temperature, a sensation of cold takes place, even though the lowest point of change be such as, when permanent, becomes oppressive by its heat. Thus, that a change of the external air from 90° to 85° , is accompanied by a sensation of cold, though this speedily goes off, and is succeeded by a sensation of heat, if the temperature of 85° be continued*. This observation ought, I think, to be restricted to degrees

ceeded all others in heat—We may infer this from one of the best of Martial's epigrams.

*Si temperari balneum cupis fervens,
Faustine, quod via Julianus intraret,
Roga, lavetur, Rhetorem Sabinæum;
Neronianas is refrigeret thermas.* Lib. iii. 25.

A person was employed at one time to regulate the heat of the baths, but in Seneca's days this had fallen into disuse. (*Epist.* 37.) This rage for bathing was checked by Adrian, and regulated by Severus. The fashion of heating the baths to this extraordinary degree did not continue—We have the authority of Galen, who flourished soon after Seneca and Plutarch, that in his days very hot baths were no longer in use. See *Hier. Mercur. lib. i. cap. 10.*

* See *Dissert. Med. Inaug. De frigore*, by A. Cullen, published at Edinburgh, 1780, (p. 8 and 9.) which contains the best general view of the doctrines of the author's celebrated father on the operations of cold.

degrees of heat inferior to that of the human body, for it does not seem true when applied to superior degrees of heat. If, for instance, a person is immersed in a bath heated to 104° , and a quantity of cold water be speedily admitted, so as to lower it to 98° degrees, a sensation of a very agreeable nature takes place, not however to be denominated cold—Where the actual heat of the body has been raised, and the circulation accelerated, a much greater reduction of the temperature of the bath must take place before it becomes cold to the sensations. In the same manner a person under the steady heat of fever, (102° to 106°) suddenly throwing off his bed clothes, and exposing himself naked to the external air, does not perceive it cold, but merely cool, a sensation too which goes speedily off. It is true there are some exceptions to this in fever, of which I have given one instance in detail. (*See page 46.*)

The manner in which our sensations are affected by changes of temperature, is a subject of importance, as well as of difficulty, for without a more precise knowledge of this, the action of temperature on life cannot be understood. In the state of health, I believe, however, that our sensations of heat and cold may be reduced to general principles.—But in certain of the Neuroses, as
well

well, as of the Pyrexiaë, these sensations are much perverted, and even in health they are much influenced by constitution, habit, and situation. To enlarge on this point would however lead into the general subject of the operation of cold on the living system, for which I am not sufficiently prepared, and which I have studiously avoided.

CHAP. XIII.

*Use of the cold Bath in convulsive Diseases—
 Substance of a Paper read before the Medical
 Society of London—Use of the cold Bath in
 Insanity—General Remarks.*

IT might now be expected that some observations should be offered on the operation of cold water *x should* applied to the surface, or received into the stomach, in the various cases that have been related; but besides, that it is advisable that the reader should form his conclusions in a great measure for himself, we shall be better prepared for such general reasonings as may suggest themselves, when we have observed the operation of the cold bath on convulsive affections, and on some kinds of mania. — That the reader may have the whole of what I have to offer in one view, I shall here

insert the substance of a paper read before the Medical Society of London, May the 10th, 1790, and published in the third volume of their memoirs.

I shall first speak of Tetanus. For this disease, so terrible in its progress, and so generally fatal in its issue, several new remedies have been proposed within the last thirty years, and each in its turn has had some share of public confidence. But opium, mercury, the cold bath, and wine, are those only, which seem intitled to any reputation, and of each, it has been my fortune to have had some experience. This experience I proceed to give with all the clearness and faithfulness in my power.

1. George Gardner, a soldier in the Staffordshire militia, was put under my care by his officers, on the 20th of February, 1781. About a fortnight before, after severe dancing and hard-drinking at a country wedding, in which he had been employed two days and nights, he fell suddenly into a fit, which lasted an hour and a half, during which his consciousness was abolished. On recovery, he was affected with slight twitches, which gradually encreased, and were afterwards followed by fixed spasmodic contractions
in

in different parts of the body, but more affecting the left side than the right. He had, when I saw him, all the symptoms of tetanus. The head was pulled towards the left shoulder, the left corner of the mouth was drawn upwards, the eyes were hollow, the countenance pale and ghastly, the face and neck bedewed with a cold sweat; but his most distressing symptom was a violent pain under the ensiform cartilage, with a sudden interruption of his breathing every fourth or fifth inspiration, by a convulsive hiccup, accompanied by a violent contraction of the muscles of the abdomen and lower extremities. He felt on this occasion as if he had received an unexpected blow on the scrobiculus cordis. Before I saw him, he had been bled and vomited repeatedly, and had used the warm bath, not only without alleviation, but with aggravation of his complaints. The three first remedies mentioned were used here in succession, viz. opium, mercury, and the cold bath.

He first took a grain of opium every other hour, afterwards a grain every hour, and at last two grains every hour; but he grew worse and worse during the two days on which this course was continued. The spasms extended to the back and shoulders, the head was at times retracted,

tracted, and the muscles of the abdomen partook of the general affection. Being no longer able to swallow the pills, he took no medicine of any kind on the night of the 22d, in the course of which general convulsions came on, and returned once or twice in every hour. The tincture of opium (liquid laudanum) was now directed to be given, and an ounce of the quicksilver ointment to be rubbed in on each thigh. In twenty-four hours he took two ounces and a half of the tincture without sleep, or alleviation of pain. The dose being increased, in the next twenty-six hours he swallowed *five ounces and a half* of the laudanum, a quantity which, at that time was I believe unexampled. He lay now in a state of torpor. The rigidity of the spasms was indeed much lessened, and the general convulsions nearly gone; but the debility was extreme; a complete hemiplegia had supervened; the patient's eyes were fixed, and his speech faltering and unintelligible.

As this young soldier appeared on the utmost verge of life, it seemed no longer safe to continue the laudanum, which had relieved spasm only in so far as it had brought on general paralysis. In permitting this medicine therefore, we gave small doses of camphor from time to time in a liquid form, but the chief attention was directed to supporting

porting the strength by such nourishment as could be swallowed. Gruel, with a small quantity of wine, was ordered for him, though with much caution; for at that time I was not instructed with what safety and efficacy this last article might have been administered. For the next six days he seemed to revive: the general convulsions ceased off, though the twitchings and convulsive hiccup continued. But on the night of the first of March he was seized, during sleep, with a convulsion as severe as ever, and this was followed by a return of all his symptoms with their former violence. The jaws were indeed more completely locked than before, deglutition was become impossible, and the pain under the ensiform cartilage was so extreme, as to force from the patient the most piercing cries. At this time the effects of the quicksilver ointment were apparent in the fetor of the breath, and in a considerable salivation.

Had poor Gardner been a man of any rank, or indeed had he been surrounded by his family, it is most probable that we must now have abandoned him to his fate. But our proceedings being obstructed neither by the prejudices of ignorance, nor the weakness of affection, another, and last effort for his life was resolved on. Having heard that the cold bath had been employed with success

success in tetanus in the West Indies, particularly by Dr. Wright of Jamaica, and Mr. Cochrane of Nevis, and this practice corresponding with certain speculations of my own, I had recourse to on this occasion with some little confidence. With the consent of his officers, Gardner was carried to the public salt-water baths of this town, the temperature of 36°. Fah°. and thrown headlong into it. The good effects were instantaneous. As he rose from the first plunge, and lay struggling on the surface of the water, supported by two of his fellow-soldiers, we observed that he stretched out his left leg, which had been for some time retracted to the ham. But his head did not immediately recover the same freedom of motion, and therefore he was plunged down and raised to the surface successively for upwards of a minute longer, the muscles of the neck relaxing more and more after every plunge. When taken out, we felt some alarm: a general tremor was the only indication of life, the pulse and the respiration being nearly, if not entirely, suspended. Warm blankets however had been prepared, and general friction was diligently employed. The respiration and pulse became regular, the vital heat returned, the muscles continued free of constriction and the patient fell into a quiet and profound sleep. In this he continued upwards of twelve hours, and when he awoke, to the astonishment

every one, he got up and walked across the room, complaining of nothing but hunger and debility. The convulsive hiccup indeed returned, but in a slight degree, and gave way to the use of the cold bath; which he continued daily a fortnight longer; and in less than a month we had the satisfaction of seeing our patient under arms, able for the service of his country.

That the opium, though it failed in effecting a cure, had considerable influence in mitigating the disease, and prolonging life, is, I think, apparent.

That the mercury had little effect, is clear from the second convulsions coming on soon after the salivation appeared.

The success of the cold bath in circumstances apparently desperate was not lessened by bad effects of any kind. Though the patient was in salivation when thrown into it, yet this was not stopped suddenly; it lessened indeed immediately, and soon disappeared, but without any of those bad consequences so well described by Sir John Bluester and Dr. Dobson. Subsequent experience has taught me to attribute some part of the suddenness of the benefit obtained in this instance, to a circumstance that distressed me much at the moment.

moment. The very instant that we were about to immerge poor Gardner, he was seized with general convulsion. We hesitated—but kept our purpose, and happily plunged him into the water with the convulsion upon him. I am also inclined to think, that our success is in part to be ascribed to the powerful, general, and sudden application of the remedy, and in this view of the subject, immersion is in certain circumstances perhaps preferable to affusion.

2. Soon after this I was sent for by a poor woman, who, in consequence of a difficult labour, and, as she imagined, of local injury in some part of the uterus, was seized with the spasmodic, and locked jaw. She was immediately taken to the cold bath, and thrown into it in the same manner as the former patient, and with similar good effects. The spasms disappeared, and though they afterwards returned in a slight degree, they gave way entirely to a second immersion.

In the first of these cases I was assisted by Mr. Walker, surgeon's mate of the Staffordshire militia; and in the second case the effects of the cold bath were witnessed by him and my friend Mr. Park.

So far of the idiopathic tetanus; my experience of the effects of the cold bath in the disease, originating in wounds, is neither so satisfactory nor so complete.

3. The first case that occurred to me was that of a master of a vessel returning from sea in the year 1784. The injury had been received on the leg, but the wound had little inflammation on it, though the patient was so far advanced in the disease, as to go off in a general convulsion, before any remedy could be tried.

4. The second case seemed rather an instance of locked jaw, properly so called, than of tetanus. The injury having been received on the jaw itself, the affection was confined to the muscles of that part, and the disease seemed to be the trismus inflammatorius of Sauvages. It gradually yielded to bleeding, and the topical application of warm vapour, the cold bath producing no good effect.

5. In the third case, we contented ourselves with pouring cold water in successive buckets on the patient's head, partly because this was the most convenient method, and partly because it is that employed by Dr. Wright, whose paper on the subject, in the sixth volume of the Medical Observations and Inquiries, had by this time appeared.

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While we were busy in this way, and flattering ourselves with some appearance of success, the unhappy patient suddenly threw himself on his back, and a quantity of water entered his mouth. The effects were highly distressing. The muscles of deglutition were unable to carry down or to expel the fluid, and such convulsions took place, as led us to expect instant death. He recovered, however, so far as to regain his former situation, but the effects of his sufferings on the bye-standers prevented our continuing this practice, and he was carried off a few hours after.

6. A fourth instance of this disease I saw about two years and a half ago. There was no room for new practice, for the patient was too far advanced. He had been treated with opium and mercury as usual, and as usual died.

These cases afforded little inference either in favour of or against the use of the cold bath in tetanus arising from wounds, but they are stated shortly, that the account of my experience in this disease may be fair and complete.

It is worthy of remark, that the use of the cold bath in tetanus, is a practice of very ancient date. Dr. Wright, whose precision and candour are exemplary, mentions, that he received the

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first hint of it from Dr. Lind of Haslar. Hippocrates, however, was acquainted with it, as appears by Aphor. 21. lib. v. It is also noticed by Avicenna. lib. iii. cap. 7. and in the collection of Schenkus, p. 120. An account of the success of this practice in two cases of tetanus is quoted from Jason, cap. 21, *De morbis cerebri, ex Valesio*. Hippocrates, indeed, and Avicenna, restrict the use of this remedy to the young and corpulent, and to the summer season. They also confine it to cases in which the disease has not originated in local injury; for it appears by another of his aphorisms, that the father of physic thought convulsions supervening to a wound always fatal*. The experience of Dr. Wright encourages us to extend this remedy to tetanus originating in wounds, but if it should fail, we are still not to despair, as the following case will testify.

7. In July 1787, a labouring man was brought into our infirmary with a wound in his hand. He was under Mr. Park's care, and the sore was nearly healed, when an alarming rigidity appeared in the motion of the jaw. Opium and mercury were immediately prescribed,

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with

* Hippoc. Aph. lib. iv. sect. v. aph. 2.

with which were conjoined, first the cold, and afterwards the warm bath. Mr. Park, however, finding the disease to proceed with the most unfavourable omens, called a consultation, at which, in the absence of the rest of our colleagues, we agreed to make a trial of bark and wine, on the authority of Dr. Rush, of Philadelphia, whose paper on this subject, in the second volume of the American Philosophical Transactions, had just appeared. We had, however, little or no hope, for the disease was far advanced, and its progress had been very rapid. At this time the jaw was not only rigidly contracted, but the spasms had extended to the neck and back, the pain under the ensiform cartilage was most acute, and twice or thrice in every hour he was seized with general convulsions, each of which lasted about half a minute.

The patient was, however, a man of a vigorous mind, and in his perfect senses; and his danger was not concealed from him. Death, he was told, must be the inevitable consequence, unless he swallowed wine in large quantity, but this we verily believed would save him. At first bark was infused in the wine, but he could not swallow the mixture, and therefore we trusted to wine alone. It was wonderful to see the exertions which this poor fellow made. If the liquid was offered to him

him at an improper time, the effort of deglutition brought on a general convulsion; nay a general convulsion was the consequence of advancing it at such a time towards his head. But watching the remission of the spasms, he was able to swallow a table spoonful or more at once, he himself giving the signal when the wine should be administered.* In this way, through the opening made by the lapping of the upper jaw over the under one, he drew up and swallowed a quart of port wine in the course of two hours: at the end of which time he thought himself refreshed, and was encouraged to proceed. At the end of twenty-four hours he had finished his third bottle, and at this time it was evident that the down-hill progress of the disease was checked. Though the pain under the ensiform cartilage (the most excruciating of all the symptoms of tetanus) was little abated, yet he felt himself more able to bear it; and the general convulsions were certainly less frequent and less severe. We continued our plan with patience and vigour, but for a long time life and

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death

* This circumstance suggested to me the similarity between this case and hydrophobia. I noticed it to my respected friend Dr. Percival, who has mentioned it in his essay on that disease. *Vide* Percival's Essays, vol. ii. p. 366, 4th edition. I differ however from this experienced physician in his supposition that the two diseases are the same, or of the same origin.

death seemed to hang in equal scales. On the fifteenth day of this course he was affected with a slight nausea and vomiting, which soon went off, and thenceforward recovery seemed more apparent. It was not however till he had been forty-two days under this treatment that his safety could be ascertained, and during this time he swallowed a hundred and ten bottles of Port Wine.

In this case bark was administered along with the wine towards the latter period of the complaint; and during the whole of it, gruel or milk in the quantity of a quart a day, or upwards, was given by way of nourishment. The milk, however, he preferred, as agreeing better with the wine, and tending, as he conceived, to allay a burning sensation at his stomach, the effect of an acid ferment there.

Though the wine was given in such quantity, yet it never produced any symptom of ebriety—it soothed the irritation of his nerves, and comforted his mind, and, without increasing the frequency of his pulse, it augmented his strength.

Every night he took from sixty to a hundred and twenty drops of the tincture of opium, and with the wine, this small dose seemed to have a
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more composing effect than thrice the quantity taken before he began it.

To allay the pain under the ensiform cartilage, cloths, dipped in æther, were applied externally with good effects. As I stood by him one day, pressing the dossils with my open hand to the scrobiculus cordis, he expressed himself relieved by the force which I applied. The same relief seemed to follow from a gentle and uniform pressure on the parts suffering from spasm. In consequence of this, rollers were applied round each thigh where spasmodic twitchings had been very distressing, and afterwards, at his own request, a general pressure of the same kind was applied to the back and abdomen. These bandages he would not suffer to be removed, but we poured æther on them occasionally, over the parts most affected by spasm, guarding in the usual way against the cold produced by too speedy an evaporation.

At length our patient recovered. He is now (1790) a watchman, and calls on me occasionally. He complains of a stiffness at times in the muscles of his back, and though naturally a strong and healthy looking man, his features retain the indelible impression of his disease. His eyes appear hollow, his face sharp and pale, his cheeks and lips are
skinny,

skinny, and the masseter muscles hard and shrivelled.

I have lately had an account of a case of this kind which occurred in a sailor on the coast of Guinea, who recovered under the care of Mr. Robson, an ingenious surgeon in the African trade, from a state apparently desperate, by a very liberal use of bark and ardent spirits.

To conclude this part of my subject, I have known two instances of this disease in horses both originating in wounds; the first of these was treated in the method first described. The cold bath was used repeatedly, and large doses of opium employed in the form of clysters; but the disease terminated fatally. The second of these occurred in a horse of Dr. Camplin's about a twelve-month ago in his journey to Bath. The doctor informs me, that he gave him wine and opium, mixing sixty drops of laudanum with every pint of wine, and by this practice he had the pleasure of recovering him, though not till he had drunk as much wine as he was worth. Brandy or gin may perhaps be as successful in such cases.

It may be supposed, that these instances of success in the use of the cold bath in tetanus, would

would lead me to employ it in other convulsive disorders. This has accordingly been the case. Of the use of the cold bath in such disorders I can speak with some confidence, as my experience of it is now of several years' duration.

In the convulsions of children I have found the cold bath a most useful remedy, whether the disorder originated in worms or other causes. I have seldom known it to fail in stopping the paroxysms, at least for some time, and thereby giving an opportunity of employing the means fitted to remove the particular irritation. I must however observe, that in early infancy I have used it with caution, and generally by affusion, tempering the water when the weather was cold. I have in general made the application of cold in this way sudden and transient, have employed means to secure re-action, and have avoided the remedy entirely in all cases where the vital energy seemed much exhausted. With these precautions I have seen great success attend this remedy in a variety of instances. Minutes of several of these I have preserved, but I mean to give one only in detail.

John Slater, aged eight years, came under my care in the month of January, 1782. About two years before, while at play, he was
seized

seized suddenly with a convulsion which continued for half an hour, and had returned ever since at short intervals. Various means had been employed for his relief, but without success; the fits were become more and more frequent, a hemiplegia had supervened, and the intellect was apparently lost. For twenty-four hours he had lain in a state of insensibility, motionless on the left side, and the muscles of the other side only agitated by convulsive tremors. We put him immediately into a tub of cold water, which instantly stopped the paroxysm, and threw him into a deep sleep, out of which he awaked after two hours with a shriek, and fell into convulsions as before. The cold bath was repeated, and afterwards continued daily, present relief being always obtained by it. The interval was employed in administering the tin-powder, which was worked off with calomel, but no worms were observed to be discharged. After several days the convulsions returning, though with a considerable abatement, I became dissatisfied with the mode in which the bathing had been performed, the size of the tub employed never having admitted of sudden immersion. On this account we had the child conveyed to the public bath, into which he was thrown headlong, his father being stripped in the water to receive him. The temperature of the bath was 43° Fahrenheit. He was repeatedly
plunged

plunged down and taken up for half a minute; was taken out of the water free of convulsion, fell immediately into a profound sleep, and awaked clear of complaint. In these respects this case resembles that of Gardner, first described. It resembles it also in this respect, that convulsion had taken place at the moment of immersion. The paralytic weakness of the side remained however for some time, but by the continued use of the bath, it was at length entirely removed; and the powers of the mind, which had been totally suspended, gradually returning, at the end of six months were perfectly restored.

He continued well upwards of twenty months, was healthy and vigorous in body, and in the acquirement of knowledge remarkably accute. But on the 30th of December, 1783, his mother having struck him and frightened him much, he was seized in the night during sleep with general convulsion, in which he continued several hours without intermission. Being again called to him, I employed the same remedy, but at the time of immersion the convulsion was not on him. Sleep and refreshment followed as before, but the paroxysm returned in the evening, though in a slighter degree. However by perseverance in the daily use of the bath, and throwing him
twice

twice into the water with the *convulsions upon him*, in six days his health was restored.

I lament over the fate of this amiable boy. He continued from this time three years and a half in perfect health; he grew strong and tall, and shewed great affection of temper and sensibility of mind. But being violently agitated with fear and grief, he fell again into convulsions in the month of July, 1787. I was sent for as usual, but was absent from town at a considerable distance. Having on former occasions launched him myself into the bath, his mother, a superstitious woman in a low walk of life, would not permit this remedy to be used till my return. He lay for thirty hours convulsed and senseless, and the first intelligence I had of his illness, was accompanied by the account of his death.

This case differs materially from the convulsions of early infancy. I give it therefore not as an instance of these, but as a specimen of a disease affecting children from four to twelve years of age, and which, though not very uncommon as far as I can judge, has not yet found its place in any systematic work that I know of. The nomenclature of convulsive diseases is indeed very imperfect, and the terms we employ in describing them are by no means precise. Those who have
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ben tetanus and the disease last mentioned, will now, that what is called convulsion in both instances, is in the one case and the other a very different affection. The convulsion of tetanus is sudden and violent contraction of all the voluntary muscles, throwing the frame into strong contortions, but lasting only a minute at most, and seldom so long. The patient, where the case terminates fatally, probably dies at last, from spasmodic affection extending to the heart, or sometimes perhaps, from the suspension of respiration. The convulsion in the other case though it begins with violence, soon subsides, leaving the body in a state approaching to coma: and though the separate fibres of many of the muscles keep in constant vibration, and the turbulence of general convulsion returns occasionally, yet in the intervals the respiration is natural, and the pulse little disturbed; voluntary motion and consciousness, are, however, completely suspended.* If this state continues long, the violent commotions return more frequently, the breathing becomes laborious, the strength is at length exhausted, and the patient dies with the symptoms of apoplexy.

I conclude

* This species of convulsion has been denominated *clonic* in opposition to *tonic*, but with little propriety. All convulsions may be said to be clonic.

I conclude these details with a few general remarks.

1. It appears to me that the efficacy of the cold bath in convulsive disorders is much promoted by being employed during the presence of a convulsion. How I came to be led into this opinion may be seen in the case of Gardner. Much experience is required to establish this as a general truth, and I give it as an opinion only. As madness is, I think, best combated in the height of the phrenzy, so I believe convulsive diseases are most capable of remedy in the agitation of convulsion. I seem to have a glimpse of a general principle in nature that connects the facts so apparently different; but the ground is not yet firm enough for the foundation of a theory.

2. It may however seem to strengthen the above remark, that in spasmodic diseases, which rise not to general convulsion, the cold bath seems to be of inferior efficacy. In Chorea Sancti Viti I have tried it frequently, but never found it of any service. This is one of the few diseases in which electricity is of decided advantage.

3. In the hysteric paroxysm, the cold bath or indeed the plentiful affusion of cold water,

an infallible remedy. Those who suppose that the terror it occasions ought, in this case, to prevent our having recourse to it, are, in my opinion, mistaken. Though the hysteric paroxysm be the offspring of passion, it is never occasioned, I will venture to assert, by the passion of fear. A sense of danger will always, I believe, prevent it; or indeed a powerful dread of any kind. I have known a tub of cold water kept in readiness, with the certainty of being plunged into it on the recurrence of the paroxysm, cure this disease, without the remedy being ever actually tried. I know the hysteric paroxysm often takes place when *danger is over*, but that is another case.

4. I have tried the cold bath in the epileptic paroxysm, but my experience of its effects is as yet (1790) too recent and too imperfect to be detailed. I am not so sanguine as to expect that experience will accumulate rapidly on this subject. Many there are who will think my practice too hazardous to be copied, even after this account of its success: This relates especially to the use of the cold bath in convulsive disorders in private practice; but in hospital practice, and in the practice of the fleet and army, the same objections do not present themselves; and in these departments of the profession it may be expected that its effects will be ascertained.

Dr.

Dr. Girdlestone, in his account of the diseases among the troops in India, mentions, that hot Madeira wine is given with success in the tetanus which occurs in that climate; but he tells us, though not from his own experience, that the cold bath has been very unsuccessful. To what circumstance it may be owing, that a remedy which has been so efficacious in the West Indies should have failed in the East, does not appear, though if the circumstances under which it was employed were fully related, the difficulty might perhaps be explained. It may, however, be observed, that in the sultry climate of India, where the human frame is greatly relaxed, it will not be safe to use the cold bath with the same freedom as in more northern regions. And it will undoubtedly be more safe in any future trials of this remedy in that country, to use the method of affusion employed by Dr. Wright, and so successful in his practice in a similar climate, instead of immersion, which it appears was employed in the cases mentioned by Dr. Girdlestone.

1798. Since the above was written, I have seen three different cases of tetanus arising from wounds, in which the affusion of cold water was employed alone, and in all these the disease terminated fatally. I have seen a fourth case in which the affusion of cold water was conjoined

with the liberal use of wine and opium, where the patient recovered. And I have seen two other cases in which the cure was trusted to wine and opium alone, in one of which the issue was fortunate, in the other unfortunate.

It is however proper to observe, that in the three first cases, the disease was far advanced, having resisted other powerful remedies before the patients fell under my care; and as the power of deglutition was nearly lost, the affusion of cold water was employed under desperate circumstances, as a *dernier ressort*. It was besides impossible to use this remedy to any extent, for the powers of life were too much exhausted to sustain the continued application, or the frequent repetition, of so violent a stimulus.—There is also in the advanced stage of tetanus, an objection to the use of a remedy that requires much change of posture. In such cases, every exertion of the will on the voluntary muscles has a tendency to induce a general convulsion.—From a consideration of all these circumstances, and from more extensive experience, I should now be disinclined to the use of the cold bath in any of its forms, in the symptomatic tetanus, unless in the earlier stages of the disease, when the vigour is less impaired, and the convulsive actions less firmly catenated.

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The practice, first introduced by Dr. Rush of giving wine in large quantities in tetanus, of which a successful case is detailed in this chapter, seems to me to be fully confirmed, as a most important part of the treatment of this very dangerous disease. The wine ought however to be combined with considerably larger doses of opium, according to my later experience, than in the case already referred to.

I have now employed the cold bath in other convulsive diseases for fourteen years, and on the whole, it has been attended with considerable success.—One conclusion, of rather a singular nature, was drawn from the cases formerly given, which my subsequent experience has uniformly corroborated:—"That the efficacy of the cold bath in convulsive disorders, is much promoted by its being employed during the presence of convulsion;" or, as I would now express it, *that the benefit derived from the cold bath in convulsive diseases, depends on its being used in the paroxysm of convulsion; that its efficacy consists in resolving or abating the paroxysm; and that when this effect is produced, the return of the paroxysm is greatly retarded, if not entirely prevented.* To this singular fact, for which I am unable to account, I invite the attention of physiologists: it may

may throw some light on the laws of association; a subject of such vast importance in physiology, and which hitherto has been so little understood. The following case taken from the records of our Infirmary practice, is in many respects curious, and will serve to corroborate the position just laid down.

John Westmore, aged 22, was admitted into the Infirmary on the 11th of October, 1792. In consequence of a fright, he had been seized with fits two months before, which now recurred several times every day, of various duration, from two minutes to an hour. During these his consciousness was wholly abolished. These fits occurred without warning, and were peculiar in their appearance. At first the muscles of one side were strongly retracted, then those of the other, alternately; and then the muscles of both sides, acting together, the whole trunk of the body was drawn upwards to the head; this action, resembling very exactly that of the victim of the law, suspended from the gallows in the agonies of death. His faculties of mind did not seem as yet impaired, nor the animal functions much disturbed. No medicines were ordered for this patient, but a bucket-full of cold water was directed to be thrown over him, the instant of the accession of the fit. Some circumstances prevented

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this being done for a few days, during which he went into the cold bath daily when the fits were off him, and with seeming advantage; the number of paroxysms being reduced from eight or ten in the twenty-four hours, to two or three. At length he was thrown into the cold bath on the instant of the accession of one of his fits, which was speedily terminated, and from this time forth he had no return for fourteen days, when he was discharged as cured. Westmore continued free of complaint till the beginning of February following, when his fits returned. He was re-admitted on the 24th of that month, and at that time, his fits recurred six or seven times in the day. He was again directed to use the cold bath daily—and to have a bucket-full of water thrown over him on the accession of every fit. By this practice he speedily recovered, and since that time the disease has never returned. He is now, (*March 1798*) in perfect health.

(1803) Westmore continued well till the month of January last, when he was again seized with a convulsion, and applied to me as formerly. I ordered the cold bath to be used on the recurrence of the fit, and in the interval directed him to take a grain and a half of the nitrate of silver daily. The convulsion has never returned, but about three weeks after he began this medicine

he parted with two yards of the tape-worm. He seems again, (*June 1803*) in perfect health. The nitrate of silver seems also to have succeeded with me in two cases of epilepsy.

I have used the cold bath during the paroxysm of epilepsy in various instances, but in general without the patient being roused to consciousness or sensation, and without advantage. In one case of this disease, which occurred in the hospital practice, the paroxysm returned periodically every afternoon. In this instance a cure was effected by applying a cataplasm, formed chiefly of tobacco, to the scrobiculus cordis, about half an hour before the expected return, by which a powerful impression on the system was produced, and the paroxysm of epilepsy prevented. This practice repeated several days at the expected periods, probably destroyed the diseased association, for the cure was permanent.—In this mode of application, the peculiar effects of tobacco on the nervous system are speedily produced, and it is perhaps the safest way of exciting them—because the application can be discontinued at pleasure; an advantage of great importance in the use of so deleterious a medicine, and which cannot be obtained when it is thrown into the stomach or rectum. I was induced to use tobacco on this occasion, from having observed it to succeed in the cure of obstinate

rate intermittents, when applied in the same manner, previous to the expected accession of the paroxysm. In two recent cases I have employed this remedy in a different manner. Each of the patients laboured under general convulsion, and the paroxysm had returned so frequently, as to produce continued coma, and in the bye-standers an absolute despair of recovery. I ordered a decoction of half a drachm of tobacco in four ounces of water, to be thrown up as an enema. This powerful agent penetrated the system to its very centre, roused the sensibility which the affusion of cold water, and other external impressions, could not awake; excited sickness, vomiting, and profuse perspiration, and interrupted the convulsive actions, which have never since returned. In each of these cases the recovery was altogether unexpected. The use of tobacco smoke thrown up the rectum, in obstinate constrictions of the alimentary canal, has been long established. I believe the effects may be obtained with more ease and greater certainty, by employing the decoction. The reasonings used in explaining the operation of the affusion of cold water, are perfectly applicable in accounting for the effects of tobacco; and there is reason to hope, that convulsions stopped in their paroxysm, by the one or the other remedy, are not in many cases liable to return.

In spasmodic diseases which rise into general convulsions, I have seen these convulsions rendered less frequent, and for a time seemingly diminished in violence, by medicines used during the intervals; but seldom effectually cured, except by means used during the act of convulsion, at the instant of its accession, or at the period of its expected return. Of the medicines employed during the intervals of epilepsy, the oxyd of zinc seems to me to have some efficacy; but the digitalis purpurea still more.—In consequence of its powerful influence, I have used this last medicine in many cases of epilepsy, both in private and hospital practice, of twelve of which I have preserved particular registers. *In every one of these*, it diminished the number of paroxysms, and the intervals were so much lengthened, that in several instances we entertained hopes of a cure. It did not however finally succeed in any one of them; and when, as the constitution became accustomed to its influence, the fits began to return, and we increased the dose of the medicine, its deleterious qualities, so well described by Dr. Withering, presented a danger greater than that of the disease, and obliged us to abandon it. On one occasion, we had an opportunity of observing, that pushing this medicine to the utmost extremity did not effect a cure. A female patient took it so rashly as to produce the utmost danger of her life. Her

pulse sunk to thirty-two in a minute, her heat at the axilla to 89° ;—her sight failed, her respiration became laborious, and incessant vomitings threatened immediate dissolution. She was however recovered by the warm bath and opiate clysters, but as her strength was restored, the epileptic paroxysms returned.

If it were my object to give a general view of the effects of the cold bath, or the affusion of cold water, in spasmodic affections, I might quote largely from the systematic writers of the last century, and I might detail various cases which have been furnished me by respectable living practitioners.—My friend Dr. Ford has mentioned to me the case of Mr. C. of Bristol, who was instantly relieved of an obstinate stricture of the neck of the bladder, of thirty hours duration, (during all which time not a drop of water had passed) by placing his feet on a marble slab, and dashing cold water over the thighs and legs. The effect was instantaneous; the urine burst from him in a full stream, and the stricture was permanently removed. The common remedies, particularly opium and bleeding, and each of these very largely, had previously been used in vain. I record this case with the more pleasure, as it will recal similar cases to the learned reader's memory, and indicate a numerous class of diseases of the same nature,

nature, in which this powerful remedy may be employed.

Cold water cannot be used as a drink during the paroxysm of convulsions, and of course we cannot shew the analogy between its external and internal use in these, as in other diseases. That its effects taken internally, are most salutary in a numerous class of chronic diseases, is however well known, though perhaps not acknowledged to the full extent of the truth. A considerable part of the virtue of mineral waters, is doubtless to be attributed either to the diluting quality of the pure element itself, or to the invigorating effect of cold on the stomach, and through it on the system at large. The subject is however sufficiently extensive, as well as important, to require and to deserve a distinct examination. In hypochondriacal, hysterical, and dyspeptic affections, cold water taken internally has produced the most salutary effects. Hoffman praises it in head-ach, whether arising from indigestion, or some primary affection of the nerves of the head. The following case, extracted from the first volume, p. 475, will shew the use of cold drink in certain convulsive affections.

*“ Puer hebræus duodecim annorum, a terrore
& frigore, incidit in ingentes præcordiorum anxietates*

tates, cum tussi, virium lapsu, extremorum tremore, intercurrente horripilatione & interno æstu. Tertiâ die post manus & pedes convulsivis motibus & subsultibus agitati; per horæ dimidium insistentibus. Inde magis magisque increverunt convulsivi hi motus, ut dorsum & collum aspectu horrendis incurvationibus quassarentur, idque aliquoties quotidie. Sub paroxysmo pedes frigebant, respiratio erat crebra, pulsus celer & durus, sensus tamen constabant. Post paroxysmum languebat corpus universum, appetitus vero cum somno erat integer. Adhibiti in consilio medici multi; exterorum etiam consilia conquisita. Plurimi verminosam progeniem subesse causam rati, aliqui per terrorem invecum esse malum putarunt. Sed data, tam ex illorum scientiâ anthelmentica, quam ex horum præscripto antepeleptica, adhibitis simul balneis, fuerunt prorsus frustranea. Malum potius perstitit idem, quin subinde factum acerbius. Tandem miser aquam fontanam frigidam, ad unam vel dimidiam mensuram, quotidie per vices bibere jussus est. Quo intra 14 dies motus isti convulsivi paullatim compositi, nec in hoc usque tempus iterum infestarunt." This simple and successful practice deserves to be imitated.

Since the last edition of this volume, two cases of tetanus have occurred to me in private practice,

practice, both arising from wounds, both extremely formidable in their symptoms, and both terminating happily. I think it my duty to give some account of them here, as exhibiting the combined effects of the different remedies employed in the former cases, and presenting my last and most successful treatment of this terrible disease.

Mr. A*****, forty-four years of age, and of a vigorous constitution, in returning home in the evening of the 26th of January, 1799, fell into a cellar and received an extensive wound in his leg. He was attended by my friend Mr. Minshull, now surgeon to the Liverpool Infirmary, who applied the proper dressings, and gave him some laxative medicine. The wound not healing kindly, bark was ordered on the 5th of February, which he continued till the 10th, on the evening of which day, a stiffness in the jaw first appeared. On the 11th he began to take opium, and on the morning of the 12th, when I first saw him, he had taken six grains of this medicine, but his disease was making rapid progress. We therefore encreased the quantity of opium. He was directed to take one grain every two hours, and three grains at once at bed-time; and he was desired to take at least one bottle of
wine

wine in twenty-four hours, with as much gruel and jelly as his stomach would bear.

Though Mr. A. now swallowed with great difficulty, and at intervals only, he was able to conform in a great measure to these directions. He got down twelve grains of opium, and rather more than a bottle of wine, daily, with a sufficient quantity of jelly and milk (which last he preferred to gruel) for the ordinary demands of nutrition. The progress of the symptoms was somewhat retarded, but not suspended; pain and stricture came on under the ensiform cartilage, and dossils, dipped in æther and laudanum, were kept constantly applied with a gentle pressure to the scrobiculus cordis. The same application was occasionally made to the muscles of the thighs. On the sixteenth, the disease was however, evidently increased.—The muscles of the neck and back were now powerfully constricted, and the pain under the sternum, produced from time to time those sudden and violent contractions, which convulse the whole system, and render the breathing at the moment difficult and irregular. There seemed nothing to hope unless some more powerful means could be adopted. In addition to the remedies already employed, we determined therefore to have recourse to the affusion of cold water on the surface, but having been somewhat discouraged

discouraged by the cases mentioned, *p.* 143, I desired the water to be raised to 75° of Fah^t, which is nearly the temperature of the springs, and of the sea in the West Indies, where Dr. Wright had employed this remedy with so much success. Mr. A. found considerable and immediate relief from the affusion, which, at his own request, was constantly repeated whenever the symptoms were most severe, and always with sensible and instant benefit. From this time the symptoms became stationary: in a few days there was an abatement of their violence, and under a continuation of this treatment, he finally recovered. The case was however a considerable time doubtful; it was not till after the expiration of twenty days, that we could consider the recovery as certain. He took during this time, on a medium, twelve grains of opium, and nearly three pints of wine in the twenty-four hours, and had the cold affusion between three and four times daily. The costive effects of opium were obviated by calomel, by which, at one time, his gums were slightly affected. From the beginning of April, the opium was reduced to one grain at bed time, and the bathing was entirely omitted. He took in all, two hundred and seventy grains of opium, and used eighteen ounces of the tincture of opium in embrocation, with twice the quantity of æther. The wound in the leg, which was deep, continued

ained open till the month of June, when it healed kindly. Mr. A. is now living and in perfect health.

The other case is still more remarkable.

In the beginning of April (1803) a splinter ran in under the nail of the middle finger of Mr. Payne, of the house of Wood and Payne, merchants, and pierced as far as the first joint: a part of the splinter was removed, but a part remained in the wound, and the finger continued painful. He however thought slightly of the circumstance, and did not intermit his usual occupations. On the 11th of the month he had a slight stiffness of the jaw, which increasing, he called on Mr. Minshull, by whom my assistance was requested. I saw him first on the 13th, and the symptoms were then increasing hourly. Mr. Minshull had removed the remaining part of the splinter, and taken off the nail entirely. A poultice had been applied to the finger, which was now easy. The local irritation speedily ceased, and the wound healed in a few days. But the affection communicated to the system, was not in the slightest degree alleviated by the removal of the irritation which originally produced it, a fact which is consonant to invariable experience in this disease, and which points out the impropriety of having

having recourse to amputation for the purpose of removing this irritation, as has been too often practiced.

The symptoms of Mr. P. had the most formidable appearance, and encouraged by our success in the preceding case, we adopted nearly the same treatment, but with greater boldness. Not being able to swallow opium in the form of pills, he took a mixture, containing about one hundred and sixty drops of the tincture (in doses of about twenty drops at a time) every twenty-four hours; an embrocation of the tincture, with twice the quantity of æther, was applied externally to the pericardiacus cordis, and to such other parts as were most painfully constricted, and on such parts we also rubbed in an opiate ointment, though not in any great quantity; for not finding immediate relief from it, he preferred the embrocation. He was directed to drink wine freely, and to swallow as much nourishment as he could bear. And the cold affusion was directed to be tried, and to be repeated, according as he might find benefit from it, and the state of his disease might permit the exertion requisite in using it. Mr. P. was made fully sensible of his situation, and of the exertions required to give him a chance of recovery. Being in the vigour of life, and of great bodily strength and resolution, the exertions he made were very uncommon.

uncommon. He could swallow at intervals only, and for several days never, but when turned on his face. His upper jaw was rigidly shut; yet as it lapped over the under one, he drew up the medicine through the orifice thus produced, and a much larger quantity of wine and nutriment than was expected. An accurate journal was kept by his attendants, from the 13th of April to the 11th of May, of every circumstance respecting his case, which extends to forty-two pages 4^{to}; and from this it appears, that in the interval of time just mentioned, he drank, mixed with nourishment, and by itself, the extraordinary quantity of a hundred and forty bottles of wine, being five bottles of Madeira a day, besides some ale, and several gallons of brandy. From the 13th of April to the end of that month, he took, one day with another, a hundred and fifty-five drops of laudanum daily, being in all five ounces and six drachms; and used during the same time, twenty-seven ounces in embrocation, with twice the quantity of æther. He also used three drachms of powder of opium in ointment. He left off the use of laudanum about the beginning of May, but continued the use of wine till the 10th of the month, when, agreeably to our advice, the spasmodic symptoms being gone, he abandoned it. The quantity of wine drank by Mr. P. was left very much to his own discretion. He found re-
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lief from it, and was indefatigable in his endeavours to swallow it, by which means he got down this immense quantity, with an abundant proportion of liquid food; chiefly strong broth, eggs, and milk. For a considerable time he swallowed two gallons of strong broth every twenty-four hours!

In the use of the cold bath, Mr. P. was in like measure left a good deal to his own discretion. From the 13th of April to the 8th of May, when he left it off entirely, it appears, that he bathed sixty-five times, twice using the tepid, and sixty-three times the cold affusion. As he sweated profusely, we directed the water to be made milk-warm, but finding no relief from it in this way, he himself desired to return to the cold affusion, the water being from 60° to 64° of Fah°. This always relieved him for the time, and disposed him to sleep. His slumbers were however for many days of a few minutes duration only, and he generally awoke from them in general convulsions. During these slumbers his jaw was relaxed, and he was able to protrude his tongue between his teeth. In the convulsive motions under which he awoke, he bit it several times severely, and was obliged afterwards to sleep with a handkerchief crammed in between his jaws, which however did not always save him. Mr. P. was in the greatest extremity

trenity from the 16th to the 20th of April. During these four days he used the cold affusion twenty-three times, and the water employed at each time was by his own desire doubled in quantity. The efforts required to move him out of bed were most painful and difficult. Happily the muscles of his arms and shoulders were less affected than those of the rest of his body, and he was able to make some exertions with them. But it was often fifteen or twenty minutes before he could turn his legs over the side of the bed, into the tub in which he stood while the water was poured over him; and the process of his rising was always interrupted by one or more convulsions. In one of these he seized hold of the post of the bed with his hands, and stood upright, so rigidly constricted, that he could not change his position for two hours, the sweat all the time pouring in torrents over him.

Notwithstanding the violence of the disease, and the vast quantity of wine and opium which he swallowed, Mr. P.'s heat was never greater than the natural standard. The superfluous heat which these medicines ought to have produced, was no doubt carried off by the profuse and constant perspiration. Neither did I observe his heat, except in a few instances, below the standard of health, and there seems little doubt, that the wine
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and opium enabled him to sustain the cold affusion under the profuse perspirations which they excited. On two or three occasions, when he was stripped for the bath, and delayed by spasmodic affections from using it, he was seized with cold chills and general rigors. In these instances he was replaced in bed, and restored to heat by two persons stripping and lying down on each side of him. Under the treatment I have mentioned, and the profuse perspiration which attended it, the surface of Mr. P.'s body became extremely sensible to impressions, especially of cold, or for the motion of the air; his situation in this respect resembling what occurs in hydrophobia, and seems to be essential to that disease. The sensibility of his mind suffered a corresponding increase, (as is I believe usual in such cases) and his spirits became almost hysterical. He is now, (June 24th, 1803) in all respects in his usual health, excepting that some bodily weakness remains, and that there is some depression on his spirits, perhaps arising from the sudden change in his habits as to the use of strong liquors, for which he has taken a decided aversion.—It ought to have been mentioned, that both in this case and in the preceding one, great relief was obtained from the use of laxative glysters.

These cases are, I hope, decisive, in regard to the general principles on which tetanus should be

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treated.

treated. The methods which were employed, will, I trust, be one day simplified and improved. If a new case should occur, it is my intention to try opiate frictions more freely. In this way the effects of the medicine may be introduced into the system, and perhaps much of the pain, difficulty, and hazard of deglutition spared.

It is deeply to be lamented, that this disease should ever have been considered as of an inflammatory nature, and that there are even now, physicians who treat it by venesection. It is in my mind decisive against this supposition, that though the general system is so powerfully affected, the animal heat is not increased, which it uniformly is, so far as my observations extend, in all cases where there is an inflammatory affection of the system, whether originating or terminating in local phlegmonic inflammation.

The same consideration is decisive with me against the supposed inflammatory nature of hydrophobia, a notion which some respectable physicians have lately revived. Of this most singular and affecting disease, I have seen five cases, in none of which was there any increase of animal heat. All these terminated fatally. Hydrophobia has some resemblance to tetanus, but is however in my judgment, obviously and essentially different.

ent. In the case of a maid servant of my friend Mr. Roscoe, (so well known in the republic of letters) it was ascertained, that this disease proceeded from the saliva of a rabid dog, applied to the inside of the lip, without any wound having been produced—a circumstance which is in itself decisive against the identity of hydrophobia and tetanus. I have seen two dissections of persons dying of hydrophobia. In both of these there were some slight appearances of congestion in the blood-vessels of the œsophagus; and on the surface of the stomach, but such only as the convulsive motions might easily explain.

CHAP. XIV.

*Use of the Cold Bath in a Case of Insanity.—
General Reflections.*

I Have mentioned before the connexion between convulsive diseases and insanity, and conjectured that the same principles are in many cases applicable to the treatment of both.* The following case, taken from the register of our Asylum, may serve as a new proof of this connexion.

June 2, 1796. L. I. aged 32, a man of very irregular habits of life, was admitted into the Asylum, in a state of furious insanity. His disease was supposed to have been brought on by

* This connexion has been explained, and the subject very ingeniously illustrated by Dr. Darwin, (*Zoonomia*, vol. ii. c. 3.) who has arranged the whole of these diseases under one class, and if the truth of his first principles were established, might be said to have thrown a new light on one of the most curious and obscure parts of pathology.

excessive drinking. As cases of insanity, including the whole method of cure, are seldom recorded, I will give this in detail.

Very powerful methods of coercion were required in the first instance, and the bowels were afterwards cleared by a saline purgative. At night he took eighty drops of laudanum, which produced imperfect sleep, and in the morning following he was more wild than ever.

Having in some cases of insanity, found very extraordinary effects from such doses of opium as induced profound sleep, I directed that five grains of this medicine should be given every four hours, till sleep succeeded. Next day (4th *June*) it was reported to me, that he had taken twenty-seven grains of opium in the twenty-four hours. From the first five grains there was no apparent effect; after the second, he slept an hour and a half; after the third, three hours. Though the medicine had been since twice repeated, in doses of six grains each, he had no return of sleep—He appeared wild, agitated, and feeble, rather in a state of alarm than of fury. His pulse 100, and soft, with gentle and general diaphoresis.

It seemed not prudent to push the medicine farther; but having often, in cases of this nature, observed

served the soporific effects of opium to come on many hours after the medicine had been discontinued, it was directed that he should have milk as his food, and that medicines of every kind should be omitted for twenty-four hours.

The report of the 5th is as follows. "He remained in a perpetual state of alarm till eight o'clock last night; after that hour he slept, but not soundly. He has taken nine pints of milk in all, having been very thirsty. It agreed with him till ten this forenoon, when he vomited considerably. Since that time he has been more tranquil and feeble." In this state, I continued the directions to omit all medicines, ordering him a pint of wine, with two pints of gruel, daily, and such solid food as his stomach could receive and digest. This plan was followed till the 17th, when his strength was considerably restored, but his insanity as violent as ever. It was now directed that he should go daily into the bath, heated to 95°, and that he should take half an ounce of the infusion of the digitalis thrice a day, as recommended by my friend and kinsman, Dr. Currie of Chester. (*Mem. of the Medical Society of London, vol. iv.*)

He continued on this plan with little variation till the 29th; the report of that day is as follows:
 "Till

"Till the 25th the digitalis produced no obvious effect. On that day sickness came on, and his pulse was hardly perceptible. The digitalis was therefore omitted. He has been regularly in the bath, and while in it was once seized with *delirium animi*, but the mental derangement did not afterwards abate. His pulse is now about 94, feeble and irregular. He is a good deal emaciated, but more fierce than ever: he talks incessantly, with the utmost incoherence, and the association of his ideas seems wholly perverted."

In this state I ordered the digitalis and the tepid bath to be discontinued, as well as all other medicine. I directed a nutritious diet, but without wine or opium, and placed him in a situation to be as little as possible exposed to external impressions. For three days there was little change in his situation—his appetite improved, and he sweated much. At the end of this period, he had suddenly a lucid interval of several hours duration; but before I could witness it, he was again become furious.

At this time (the 16th July) I directed that he should take three drachms of peruvian bark, half a drachm of vitriolated iron, and the same quantity of aromatic powder in the form of an electuary, daily; and that twice every day, he should be

be subjected to the tepid affusion for a minute or two at a time, the water being poured gently over him of the heat of 88°.

The following is the report of the 16th *July* :
 “ The tepid affusion was continued till the 14th, but without any tranquillizing effect; the other medicines have been given regularly. On the 9th he became calm and rational, and slept the following night for eleven hours profoundly; but started suddenly from this sleep, as wild and furious as ever. This state of mind continued twenty-four hours, when he again became tranquil, and continued so till he went to bed. He slept a few hours quietly, but awoke as before, under extreme agitation, and continued fierce and raving for twelve hours and upwards. Since this time, he has had a succession of paroxysms of insanity, alternating with periods of tranquillity of mind, of nearly the same duration. In general, he goes to bed tranquil, and sleeps quietly till about three in the morning, when he awakes under the horror of some frightful dream; and this introduces his paroxysm of furious insanity, which continues from ten to fourteen hours.”

In this situation I ordered an emetic to be administered to him late in the evening, in hopes that it might prevent the return of the paroxysm.

Being

Being disappointed, I had again recourse to an opiate at bed-time; and this also proving inefficacious, I directed that he should be awake before the expected accession, and kept awake beyond its period of attack. By these means we disturbed the regularity of the alternations, but without any advantage; on the contrary, his lucid intervals became shorter in duration, and less complete.

Perplexed with these extremes, and having in mind the success of the cold bath in convulsive diseases, I ordered it to be tried on the present occasion. The insanity returning with great violence on the 21st, he was thrown headlong into the cold bath—He came out calm, and nearly rational, and this interval of reason continued for twenty-four hours. The same practice was directed to be repeated, as often as the state of insanity recurred.

The following is the report of the 30th: "The direction has been followed, and on the morning of the 23d, he was again thrown into the cold bath in the height of his fury, as before. As he came out, he was thrown in again, and this was repeated five different times, till he could not leave the bath without assistance. He became perfectly calm and rational in the bath, and has remained so ever since."

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This patient continued with us for some time afterwards, bathing every other day, and taking the oxyd of zinc in small quantities. He never relapsed, and was discharged some time afterwards, in perfect health of body and mind*.

In

* It has been contended by Dr. Darwin, that as convulsive diseases arise from inordinate action in the muscles, so maniaical diseases arise from the same inordinate action in the organs of sense:—and as the muscles, and the organs of sense, are the proper instruments of the will, the diseased actions of the one and the other are by him arranged as one class, under the title of *Diseases of Volition*. But in the system of Dr. Darwin, the actions of the organs of sense constitute our ideas; a deranged action of these organs is therefore no other than what is usually denominated a derangement of mind; in other words insanity. As therefore convulsion and madness, may be considered as the same disease differently situated; and as the seat of the disease may be changed from various causes, and extended through various sympathies and eatenations—if the proposition of Dr. Darwin, that our ideas are motions in the organs of sense, were established, we should have an explanation of that connexion between convulsive affections and insanity, and particularly of their alternations with each other, which have been observed in every period of medicine.—That several of the modes of treatment applicable to the one, are also applicable to the other, has long been known. The *arnica montana*, the oxyd of zinc, the *digitalis purpurea*, and opium, are medicines used equally in convulsions and in insanity. The case which has just been related extends this analogy, and is the more striking in this point of view because

In order however to the success of the cold bath, either in mania or convulsion, it will be readily

because the success of the cold bath was almost instantaneous, and occurred in the paroxysm of insanity, in the same manner as has been before observed of its operation in convulsion.

It is supposed by Dr. Darwin, that the motions excited by volition and by sensation, proceed in different directions; the first passing from the central parts of the system towards the superficies, the second from the superficies towards the central parts. Hence he concludes, that they are opposed to, and mutually destructive of each other. To increase the force of voluntary actions must therefore be to lessen positively, as well as relatively, the actions that arise from sensation, and the converse of this proposition is equally true. Whatever faith we may give to the hypothesis of Dr. Darwin, his inference seems agreeable to a great variety of phenomena. That the higher degrees of volition and sensation are incompatible with each other, is a truth of great importance in every part of the science of life, whether moral or physical. It was the doctrine of another great physiologist, whose praise will now be heard with approbation, since he is beyond the reach of praise, "that no two actions can take place in the same constitution, nor in the same part, at the same time;" (*John Hunter, Treatise on the Blood—Introduction. p. 3.*) and though he applied this doctrine chiefly to show the incompatibility of different diseased actions with each other, yet it seems applicable also to explain the operation of a great part of the more powerful remedies. The position of Dr. Darwin resolves itself into this more general principle of Mr. Hunter, and the operation of the cold bath in

readily perceived, that it is necessary that no considerable læsion of structure shall have taken place, and that the morbid actions be not so strong as to produce insensibility to impressions, and shall not have been so often repeated as to form indissoluble associations. It seems also necessary that the diseases in question shall be such as are susceptible of general paroxysms; a circumstance not so easily explained. Finally, it is necessary that the digestion shall not have been much impaired, or the vigour of the circulation much debilitated, lest the action of cold be too strong for the living powers. Under these restrictions, the cold bath, according to my experience, may often be applied with advantage, and always with safety, in convulsive diseases, and in insanity; for though in these diseases, even in their more violent forms, the temperature of the

in the paroxysm of madness, or of convulsion, receives a ready explanation under either doctrine. We may say with Dr. Darwin, that the powerful stimulus given to sensation destroys the previous diseased volition, or in the more general and more simple language of Mr. Hunter, that the sudden application of cold water to the surface, produces an action of the system with which the previous diseased action is incompatible. Or, what would perhaps be more prudent than either, we may defer all attempts at explanation till observation and legitimate induction have enlarged our knowledge of physiology.

body is little, if at all, increased, yet the actual heat is retained with great tenacity, as I have found by many observations.

The resistance which the fierce maniac opposes to cold, he opposes also to contagion, and to other noxious powers, as has often been observed with surprise and admiration. This resistance seems to belong to every species of increased voluntary exertion, where the functions of digestion and nutrition are not impaired. The illustration of this position would not be difficult, and the conclusions to be deduced from it are in a high degree interesting and important; but a discussion of the subject would lead us too deep into the general doctrines of life*.

CHAP.

* (1798) Dr. Marcard, of Pyrmont, in his work on the effects of warm and cold baths, entitled, *Ueber die Natur, und Gebrauch der Bader*, lately published, has quoted what I have said on the effects of the cold bath in convulsive diseases, and it appears, that he does not assent to my conclusions. Dr. Marcard supposes indeed, that the cold bath may be useful in convulsive diseases, where there is no local affection stimulating the system, but he contends, that it *must* be hurtful in cases where convulsions originate in stimuli, as worms, or crudities in the alimentary canal, teething, &c. It does not appear to me that Dr. Marcard has understood my paper perfectly, and I do not feel myself disposed to yield my experience to his speculations. The doctrine which Dr. Marcard

CHAP. XV.

An Account of the remarkable Effects of a Shipwreck on the Mariners ; with Experiments and Observations on the Influence of immersion in fresh and salt Water, hot and cold, on the Powers of the living Body.

THOUGH I have already declared, that it is not my intention to go at present into the general effects of temperature on life, yet, as I wish to

Marcard seems to maintain, that where one stimulus is already acting, the application of another must be injurious, is contrary to experience, and as I apprehend, to the laws of the animal œconomy. The work of Dr. Marcard is a valuable one, and deserves to be translated ; but it seems to be too full of those general and sweeping observations, which, though nearly banished from other sciences, continue in the writings of physicians, and serve at once as a proof and a cause of the imperfection of our art. I fear this volume will not be found free of them.

resent, under one view, what may be subservient to this purpose at some future period, I shall here insert the substance of a paper read before the Royal Society, and inserted in their transactions for 1792.

On the 13th of December, 1790, an American ship was cast away on a sand-bank that lies in the opening of the River Mersey into the Irish channel. The crew got on a part of the wreck, where they passed the night; and a signal which they made being discovered next day from Hillberry Island, a boat went off, though at a great risk, and took up the survivors. The unfortunate men had remained twenty-three hours on the wreck; and of fourteen, the original number, eleven were still alive, all of whom in the end recovered. Of the three that perished, one was the master of the vessel; another a passenger, who had been a master, but had lost or sold his ship in America; the third was the cook. The bodies of these unfortunate persons were also brought off by the men from Hillberry Island, and were afterwards interred in St. Nicholas's church yard, amidst a great crowd of spectators. The cook, who was a weakly man, died a few hours before the boat reached the wreck, but the two masters had been long dead, and this united the sympathy for their loss, with a curiosity to inquire

quire into its circumstances and causes. When the following particulars came to be known, this curiosity was increased. Both the masters were strong and healthy men, and one of them a native of Scotland, in the flower of his life, early inured to cold and hardships and very vigorous both in body and mind. On the other hand, several of the survivors were by no means strong men, most of them had been inured to the warm climate of Carolina, and what was singular enough, the person among the whole who seemed to have suffered least, was a negro.

What is extraordinary is seldom long unaccounted for in one way or other, and the death of the two masters was said to have been owing to their having taken possession of a keg which had contained cherry brandy, and which still contained the cherries;—these, it was reported they had kept to themselves, and eaten in large quantities after the shipwreck; and this having produced intoxication, was supposed to have hastened their death. Some experienced seamen were satisfied with this account, which indeed seemed very rational; for though spirituous liquors may fortify the body against the effects of heat, combined with moisture, and may perhaps support it for a short time under great fatigue, they are, I believe, uniformly hurtful, when taken un-

der severe and continued cold. Pleased to see a doctrine becoming popular, which has been so ably supported by Dr. Aikin,* and others, I believed it might receive a striking confirmation from this catastrophe, into the particulars of which I determined to examine accurately. I therefore obtained access to the survivors of the crew, and from them, but more especially from Mr. Amyat, the mate, I received the information which I required.

In repeated conversations with this intelligent young man, I learnt, that Capt. Scott, the master of the vessel, died in about four hours after the ship struck; and that Capt. Davison, the passenger, died in about seven: but that the story of their having eaten cherries infused in brandy was entirely without foundation: of this he was certain, for he saw the keg, which contained the cherries, staved, while Capt. Davison was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately washed into the sea. Mr. Amyat expressed his surprise at the early death of the two masters, but could not assign any cause

* See Transactions of the Philosophical and Literary Society of Manchester, v. 1.

for it. He said there was no liquor of any kind saved, nor any sort of food; that the whole crew were on an equality in all points, except that some were deeper in the water than others, but that the two masters had the advantage in this respect, for they sat on the only part of the wreck that was out of the sea, whereas the poor negro, who escaped almost unhurt, was perhaps deepest in the sea of any. He explained this in the following manner. When the ship struck, they cut away her masts to prevent her from over-setting, and after this she drifted over the sand-bank, into what is called a "swash," on the other side. Here she floated, and they let go their best bower anchor; but it dragged, and the vessel struck again in a few minutes on another bank. In this situation she lay some time, beating against the sand, and the sea breaking over her. In a little while Mr. Amyat saw the tar barrels, which formed her cargo, floating towards the land, and soon after her bottom parted entirely, and was carried in the same direction. Happily for the men, the part of the wreck on which they were lashed, was held by the anchor, and floated in the water, a small portion of the after part of the quarter-deck being above the surface. On this sat the two masters, generally out of the sea, but frequently overwhelmed by the surge, and at other times exposed to heavy showers of sleet and

snow, and to a high and piercing wind. The temperature of the air, as nearly as can be guessed, was from 30° to 33° of Fah^t. and that of the sea, from trials in similar circumstances, from 38° to 40° .* Immediately before the two masters was Mr. Amyat himself. As he was sitting, and the deck sloped pretty rapidly, he was generally up to the middle in the water. The situation of the rest may be supposed; some of them were up to the shoulders. They were not at any time able to change their position, but kept their legs in pretty constant motion, to counteract the cold, their arms being employed in holding by the wreck.

The master of the ship, Captain Scott, a native of North-Carolina, and about forty years of age, died first. As they were in the dark, Mr. Amyat could not see his countenance; but he was first alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing seemed to fail. At length he raised himself up in a sort of convulsive motion, in which he continued a few seconds, and then fell back dead on

* (1803) From some recent experiments I have reason to believe, that the temperature of the sea was not higher than that of the air.

the deck. This happened about eight in the evening; four hours after the ship went aground. Soon after this, Captain Davison, who was about twenty-eight, began to talk incoherently, in the same manner as the other. He struggled longer, but died in the same way, at about eleven at night. The cook died in the forenoon of the succeeding day. He was a low spirited man, and desponded from the beginning. All the rest held out, as has been already mentioned, though sorely pinched with cold and hunger, till they were taken up about three in the afternoon. Mr. Amyat said that his hands and feet were swelled and numb, though not absolutely senseless; he felt a tightness at the pit of his stomach, and his mouth and lips were parched; but what distressed him most, were cramps in the muscles of his sides and hips, which were drawn into knots. Though immersed in the sea, they were all of them very thirsty, and though exposed to such severe cold. Mr. Amyat himself was not drowsy, nor were any of the men drowsy, nor did sleep precede death in those that perished. These facts are curious.

Reflecting on the particulars of this melancholy story, there seemed no doubt, that the death of the two masters was to be imputed to their peculiar position on the wreck. Exposed to heavy showers of sleet and snow, they might suffer from being wet with
fire

fresh, rather than salt water; they might also suffer from being exposed to the cold of the atmosphere, probably seven or eight degrees greater than that of the sea.* The chilling effects of evaporation might operate against them, promoted as these must have been by the high wind; or they might receive injury from their frequent immersions in the sea, producing an *alternation* in the media surrounding them. This last supposition did not, indeed, strike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which seems fundamental, is the capacity of the living body of preserving the same heat in various degrees of temperature of the same medium, and, indeed, in media of very different density and pressure. If a definition of life were required, it is on this faculty that it might best be founded. It is known that some fluids, applied to the skin, vary in their effects according to their impregnation. In the same degree of temperature, pure water on the surface of the body is much more

* (1803.) This is now I believe a wrong supposition. I do not believe there was any difference. The water spreads over a vast expansion of sand, and as the cold had been some time great, it had probably brought both elements to the same temperature.

hurtful than water in which salt is dissolved. Seafaring men are universally acquainted with this, and a striking proof of the truth, as well as of the importance of the observation, may be found in the narrative of Lieut. Bligh. Probably the saline impregnation, may stimulate the vessels of the skin in some way that counteracts the sedative or debilitating action of the cold. At any rate, it seemed not unlikely, that some light might be thrown on this curious subject, by observing the effects of immersion in fresh and salt water, of equal temperature, on the animal heat. And this might also assist in accounting for the death of the unfortunate men already mentioned,

EXPERIMENT I.

I placed a large vessel, containing one hundred and seventy gallons of salt water, in the open air. The atmosphere was damp, and what is called raw. The thermometer stood at 44° in the air, and this also was the temperature of the water. The subject of my experiment was Richard Edwards, a healthy man, twenty eight years of age, with black hair and a ruddy complexion. The hour chosen for his immersion was four in the afternoon, about two hours after his dinner; a time appointed rather for my own convenience, than as being most proper for the purpose. His heat was 98° before undressing, his pulse 100 in the minute. He was undressed

undressed in a room where the mercury was at 56° ; and afterwards stood naked before the fire till his heat and pulse were examined again, and found as before. He then walked pretty briskly through a passage paved with stone into an open court, where the north-east wind blew sharply upon him: he was exposed to it for a minute, and then plunged suddenly into the water up to the shoulders. The thermometer, which had been kept in a jug of warm water, at the heat of 100° , was introduced into his mouth, with the bulb under his tongue, as soon as the convulsive sobbings occasioned by the shock were over. The mercury fell rapidly, and a minute and a half after immersion it stood at 87° . He remained motionless in the water, and the mercury rose gradually; at the end of twelve minutes it stood at $93\frac{1}{2}^{\circ}$. While he sat in the water, it occurred to me to examine his heat when he rose out of it into the air: I had reflected on the power that must be employed to keep up his heat in a medium so dense as water, and where an inanimate body, of the same bulk, would have cooled so much more speedily than in the air of the same temperature. Supposing that this heat-producing process, whatever it may be, might continue its operations some time after the extraordinary stimulus (the pressure of the water) was removed, I expected to see the mercury rise by the accumulation of his heat, on changing the medium of water for air, and therefore kept him exposed, naked,

naked, to the wind, two minutes after taking him out of the bath. To my surprise, although the attendants were rubbing him dry with towels, during this time the mercury fell rapidly. He was put into a warm bed, and his heat, when examined under the tongue, was 87° ; at the axilla, 89° . Frictions were used, and brandy mixed with water administered; but I found on this, as on all future occasions, the best mode of counteracting the cold was to apply a bladder, with hot water, to the pit of the stomach (the scrobiculus cordis), a fact which I think important: this being done, his shiverings, which before were severe, soon ceased, and he became more comfortable. Three hours afterwards, however, he had not entirely recovered his former heat, but by eight at night he was in all respects as usual.

I have been very minute in detailing the circumstances under which this experiment was made; some of the particulars which, at the time, I thought of little consequence, I found afterwards of importance. The experiment itself I determined to repeat as exactly as possible.

EXPERIMENT II.

On the next day, at the same hour, the same person was again immersed, as before. His pulse previously

previously was 85, his heat 100°. He had been put to bed an hour before, to save the time spent in undressing. The heat of the water and of the atmosphere 44°. The wind north-east, and strong. On this occasion, as before, there was a rapid fall of the mercury ; the following table will save words :

| | Ther. | | Ther. |
|----------------------|--------------------|----------------------|--------------------|
| 2 min. after immers. | 89 $\frac{1}{2}$ ° | 9 min. after immers. | 95 $\frac{1}{2}$ ° |
| 3 ——— . . . | 90 $\frac{1}{2}$ | 10 ——— . . . | 94 $\frac{1}{2}$ |
| 4 ——— . . . | 92 $\frac{1}{2}$ | 11 ——— . . . | 95 |
| 5 ——— . . . | 94 $\frac{1}{2}$ | 12 ——— . . . | 95 |
| 6 ——— . . . | 95 | 13 ——— . . . | 95 $\frac{3}{4}$ |
| 7 ——— . . . | 95 $\frac{3}{4}$ | 14 ——— . . . | 95 |
| 8 ——— . . . | 95 $\frac{3}{4}$ | | |

At the end of fifteen minutes he was taken out, and stood three minutes, naked, exposed to the north-east wind, at the end of which time the mercury had sunk to 88°. A draught of ale was given him, and he was put into a warm bed; in three minutes afterwards the mercury rose to 93°. An hour after his heat was 95°.

The effects produced by this alternate exposure to water and air of the same temperature, gave a new direction to my thoughts, and determined me to inquire again into this singular phenomenon. The most obvious method would have been to have prolonged the process of alternation, and re-

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plunged

plunged the person cooled by the external air into the bath ; but this was running ^otwo great a risque, unless some more sudden and certain method could be found of restoring the heat that might be lost. It was prudent, therefore, to proceed more cautiously. In the next experiment I resolved to try the methods of heating, as well as of cooling the body.

EXPERIMENT III.

On the following day, at the same hour, the same person was again immersed in the salt-water bath. His heat previously was 98° , his pulse 100. The temperature of the air and the atmosphere, as before, 44° . The mercury sunk rapidly to 90° ,

| | Ther. | | Ther. |
|-----------------|-----------------|------------------|-------------------------|
| 2 minutes after | 88° | 10 minutes after | $94\frac{1}{2}^{\circ}$ |
| 3 ————— | 88 | 11 ————— | $94\frac{3}{4}$ |
| 4 ————— | $88\frac{1}{2}$ | 12 ————— | 95 |
| 5 ————— | $90\frac{1}{2}$ | 13 ————— | 96 |
| 6 ————— | 92 | 14 ————— | 96 |
| 7 ————— | 92 | 15 ————— | 96 |
| 8 ————— | 94 | 16 ————— | 96 |
| 9 ————— | 94 | | |

He was now taken out, and stood in the wind three minutes, shivering violently. This circumstance rendered it difficult to ascertain exactly the fall of the mercury, which was, however, considerable. When he was examined in the room in which

which he undressed, it stood at 90° . He was now plunged into a fresh water warm bath, heated to $97\frac{1}{2}^{\circ}$. What is very surprising, the mercury fell two degrees.—The following table will shew the progress of the return of his heat.

| | Ther. | | Ther. |
|-------------------------|--------------|------------------|--------------|
| 1 min. after immers. in | | 5 minutes after, | 94° |
| the Warm bath, mer- | | 6 ————— . . . | 96 |
| cury, . . . | 88° | 7 ————— . . . | 96 |
| 2 minutes, . . . | 92 | 8 ————— . . . | 96 |
| 3 ————— . . . | 92 | 9 ————— . . . | 96 |
| 4 ————— . . . | 94 | | |

If the rise of heat in the cold bath at 44° , and the warm bath at $97\frac{1}{2}^{\circ}$, be compared, the first will be found more slow; but that after being sixteen minutes in the one and in the other, the heat was the same in both cases, when taken at the mouth. It must, however, be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffused.—When Edwards got out of the hot bath, he put on his clothes, and was remarkably alert and chearful the whole evening. Encouraged by the safety of these experiments, I resolved to increase the time of immersion in the cold bath, and to inquire more generally into its effects on the sensations, as well as heat.

EXPERIMENT

EXPERIMENT IV.

At the same hour of 'another day, the same person was again immersed as before, his heat being previously $97\frac{1}{2}^{\circ}$, and that of the water 42° . Wind north-east, and brisk.

| | Ther. | | Ther. |
|----------------------|-----------------|------------------|-------------------------|
| 1 minute after, heat | 90° | 12 minutes . . . | |
| 2 minutes . . . | 92 | 13 ————— . . . | |
| 3 ————— . . . | 92 | 14 ————— . . . | $94\frac{1}{2}^{\circ}$ |
| 4 ————— . . . | $92\frac{1}{4}$ | 15 to 24, . . . | $94\frac{1}{2}$ |
| 5 ————— . . . | 92 | 25 ————— . . . | 94 |
| 6 ————— . . . | $92\frac{1}{2}$ | 26, 27 . . . | |
| 7 ————— . . . | 94 | 28 ————— . . . | $94\frac{1}{2}$ |
| 8, 9, 10, 11 . . . | 94 | 29, 30 . . . | 94 |

It will be observed, that in the above table there are blanks left in the report. At such times the thermometer was taken out of Edwards's mouth, to admit of his answering the questions put to him. He said, that on plunging into the water, he felt an extreme cold, which he could not but think was partly owing to his being exposed, naked, to the wind before; that this cold diminished, and in a little while he felt comfortable, but that after a while the sense of coldness returned, though less than at first; diminishing again, but in a less degree. At length his sensations became pretty fixed. In this state, when the water was at rest, he should not even have known, by his feelings from the upper part of his chest to the pubes, that he was in the water at all. His feet and legs were very cold

cold; so were his hands and arms; and so also the penis and scrotum. He mentioned, likewise, that he felt a cold circle round the upper part of his body, though not constantly. On examining into this, I found it was greatest at first, and that it extended over the space which, from the undulations left in the bath by the plunge of immersion, was alternately above and under the surface of the water: when the bath settled, it was little felt; but by agitating the fluid, I could reproduce it at any time when the cold in the extremities was not so great as to prevent its being felt. This curious particular serves to explain a circumstance much dwelt on by Mr. Amyat, in giving an account of his sufferings on the wreck; that what he felt most severely was the cramps, in the muscles of his hips and sides, parts which from his situation on the wreck, already described, must have been alternately under and above the surge. Here I must observe, that the sea did not break over them. The wind moderated, as well as the waves, and for the last fifteen hours, they were not at any time overwhelmed, or at least Mr. Amyat himself was not. The cold never abated. Being all lashed to the wreck, they never changed their positions; the bodies of those who died occupied the space where they were originally placed. Mr. Amyat, therefore, during the whole time sat nearly up to the middle in water, but subject to the variations occasioned by the motion of the sea.

To

To return—When exposed naked to the wind, the mercury, in this case, sunk as usual five or six degrees, and his shiverings were great. Desirous of restoring his heat as speedily as possible, we incautiously heated the hot bath to 104° : but after being half a minute in it, he screamed out with pain especially in his extremities, and about his scrotum. When taken out, his shiverings almost amounted to convulsion. The bath was lowered to 88° , and he was replaced in it, and its temperature progressively, but pretty rapidly increased to 100° . He continued, however, to shiver much, his heat remaining about 90° ; but a bladder, with very hot water, being introduced under the surface of the bath, and applied close to his stomach, the good effects were instantaneous, his shiverings ceased, and his heat mounted rapidly to 98° .

All these experiments having been made on one person, I determined to repeat this last on another.

EXPERIMENT V.

R. Sutton, aged 19, of a pale complexion, and a feeble frame, was immersed in the bath, under the circumstances of the preceding experiment. His heat was previously $96\frac{1}{2}^{\circ}$.

$\frac{1}{2}$ a min.

| | Ther. | | Ther. |
|-----------------------------------|------------------|------------------|------------------|
| $\frac{1}{2}$ a min. after, heat, | 92° | 18 minutes . . . | 93 $\frac{1}{2}$ |
| 1 minute . . . | 90 | 19 ————— . . . | 93 $\frac{1}{2}$ |
| 2 minutes . . . | 88 $\frac{1}{2}$ | 20, 21 — . . . | 94 |
| 3 ————— . . . | 89 | 22 ————— . . . | 92 $\frac{1}{2}$ |
| 4 ————— . . . | 90 | 23 ————— . . . | 92 $\frac{1}{4}$ |
| 5 ————— . . . | 92 | 24 ————— . . . | 92 $\frac{1}{4}$ |
| 6 ————— . . . | 92 $\frac{1}{2}$ | 25 ————— . . . | 94 |
| 7 to 10 . . . | 92 | 26 ————— . . . | 94 |
| 11 ————— . . . | | 27 ————— . . . | 92 $\frac{1}{2}$ |
| 12 to 15 . . . | 92 | 28 ————— . . . | 92 $\frac{3}{4}$ |
| 16 ————— . . . | 92 $\frac{1}{2}$ | 29 ————— . . . | 94 |
| 17 ————— . . . | 93 | 30 ————— . . . | 94 |

Though this person seemed to bear the cold bath well, having lost in thirty minutes only 2 $\frac{1}{2}$ degrees of heat, yet when exposed afterwards to the wind, he shivered violently, and lost his heat very fast. He was put into a warm bath, heated to 96°, but recovered his heat very slowly, as the following table will shew.

| | | |
|----------------------|------------------|-----------------------------------|
| 1 minute after, heat | 88° | |
| 2 minutes . . . | 90 | |
| 3 ————— . . . | 90 $\frac{1}{2}$ | |
| 4 ————— . . . | 90 | great shivering. |
| 5 ————— . . . | 90 | here the bath was heated to 100°. |
| 6 ————— . . . | 90 | shiverings still. |
| 7 ————— . . . | 90 | ditto. |
| 8, 9 ————— . . . | 90 $\frac{1}{2}$ | ditto. |
| 10 ————— . . . | 92 | ditto. |

11 min.

| | | | |
|----------------|-------|-----|---|
| 11 min. after, | . . . | 92° | bath heated to 104°. |
| 12 ——— | . . . | 94 | |
| 13 ——— | . . . | 93 | — heated to 108°. |
| | | | Shiverings. |
| 14 ——— | . . . | 93 | a bladder with very hot water applied to the stomach. |
| 15 ——— | . . . | 94 | |
| 16 ——— | . . . | 96 | very comfortable. |

EXPERIMENT VI.

Richard Edwards, the original subject of experiment, was again immersed in the cold bath, of the temperature of 40°, and remained in it three quarters of an hour. His heat previously was 97°, his pulse 90 in the minute. The mercury fell at first to 92°, was stationary for a few minutes, and then mounted, though as usual, with no regularity. In twenty-two minutes it stood at 96°; it then began to decline, and in twenty-three minutes more, had sunk to 94°. Being exposed as usual to the wind, the mercury sunk as usual, and he shivered violently. In the warm bath at 96°, his shiverings continued several minutes, his heat remaining at 90° and 91°. In seven minutes the mercury began to rise fast, and five minutes after it was at 96°.

EXPERIMENT

EXPERIMENT VII.

The effects of forty-five minutes immersion in the cold salt-water bath, at 40° , were proposed to be tried on Richard Sutton. He was much under the impressions of fear, and his heat previously raised the mercury only to 94° . The mercury sunk, as usual, on his immersion, but to an unusual degree. It did not stop on its fall till it got to 83° , which perhaps might in part be accounted for by the extraordinary chattering of his teeth, admitting some contact of the air. It then mounted in the usual irregular way, and at the end of thirteen minutes had got to 92° . Here it stood for nineteen minutes longer, with little variation; at the end of this time it began to fall rapidly, though irregularly, and in three minutes was down at 85° . He had now been thirty-five minutes in the water, and I did not think it safe to detain him longer; we therefore hurried him into a warm bath, heated to 96° , where he shivered much.—The bath was heated gradually to 109° , and in this heat he recovered his proper temperature in about twenty-eight minutes. Being then put into a warm bed, he fell into a profuse perspiration, which left him in his usual health.

One general remark will serve for the pulse in all these experiments. It was not possible to keep

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the subjects of them from some degree of previous agitation, and this always quickened the pulse.—The natural pulse of Edwards was about 70 in the minute; but it may be observed, that it was never under 85 before immersion, and generally above it. However this might be, it invariably sunk to 65, or from that to 68, in the water; became firm, regular, and small. After being long in the bath, it could hardly be felt at the wrist, but the heart pulsated with great steadiness and due force. In the last experiment, when the heat sunk rapidly, Sutton said that he felt a coldness and faintness at his stomach, which he had not perceived before, and when I examined the motion of his heart, it was feeble and languid. In some future trials of the effects of immersion in fresh water (one of which I shall detail) the same coldness at the stomach preceded a rapid fall of the mercury; and these facts, together with the effects I found from applying a considerable heat to this part when the body was chilled with cold, convince me that there is some peculiar connexion of the stomach, or of the diaphragm, or both, with the process of animal heat. Whoever will consider the rapidity with which a dead body would have cooled, immersed in water of the temperature of 40° , may form some estimate of the force with which the process of animal heat must have acted in the experiments already recited. These experiments,

ments, however, do not coincide with our generally received theories of animal heat. The increase of heat, in fever, has led some persons to believe, that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here, however, it may be observed, that while heat must have been generated in the bath with more than four-fold its usual rapidity, the vibrations of the arterial system were unusually slow. Another, and a very beautiful theory of animal heat, supposes it immediately to depend on respiration; but in the bath, after the first irregular action of the diaphragm from the shock of immersion was over, the breathing became regular, and unusually slow. Lastly, the curious phænomenon of the heat rising, and falling, and rising again, in the bath, with the body at rest, at the temperature of the surrounding medium unchanged, is, I think, fatal to those theories of animation, which consider the living body as a mere machine, acted on by external powers, but not itself originating action, and differing from other machines only in the peculiarity of the powers which are fitted to set it in motion.* I have said that temperature of the

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medium

* I do not mean to object to the theory which refers the introduction of caloric into the system, chiefly to the decomposition of atmospheric air in respiration, but the actual state of

medium continued unchanged, but it may be supposed that the bath was heated a little during the experiments: it was so; but being exposed, with a large surface, to the open air, the wind blowing briskly over it, its heat was little altered; in twelve minutes immersion it had gained nearly one degree, and in forty-five minutes, the longest duration of any one of the experiments, it had gained three degrees. As this accession was regular, if it had been greater, it would not have invalidated the foregoing observations.

Many other trials were made on the effects of immersion in water on the human heat, which I shall speak of generally, under the general conclusions which they suggested.

The experiments already recited, suggested to me the notion, that in all changes, from one medium to another, of different density, though of the

of heat in the body, cannot safely be inferred (as some have supposed) from the quantity of air respired. Caloric is received into the system by other processes also, particularly from the food and drink in the state of digestion, and evolution of it into active heat appears to be modified by the living power on its own peculiar principles; at least it is not explained by chemical or mechanical principles, or analogies so far as we are yet acquainted with the subject.

same temperature, there is a loss of animal heat. I found, however, that this conclusion requires many restrictions.

1. My experiments being made on bodies of such very different density as air and water, do not admit an universal inference of this sort.

2. Being all made in a temperature of fifty degrees under the human heat, no certain conclusion can be drawn as to what might happen in degrees of heat much higher, where it is probable, the effects of the change, if it appeared at all, might be less striking. It should seem; however, that after a person is long chilled in cold water, the first effect of passing through the external air into the warm bath, is first a fall of heat in the air, and after this a still greater fall in the warm bath, followed, however, by a speedy rise.

The air and water being equally cold, and both 45° or under, I found the loss of heat in passing from the one to the other to be regulated in the following way.

1. If, instead of being exposed naked to the wind

wind previous to immersion in the water, the body was kept warm by a flannel covering, the mercury fell much less on the plunge.

2. If after plunging in the water, the person continued in it only a minute or two, a subsequent fall of the mercury did not always take place, on his emerging into the air. On the contrary, there was sometimes a rise on such occasions in the mercury, especially if the atmosphere was at rest.

3. In one instance, after continuing in the water fifteen minutes, in rising into the air in a perfect calm, though during a frost, there was little or no seeming diminution of the heat; while the exposure, under similar circumstances, with a north-east wind blowing sharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminishing the human heat are indeed striking, and are not in my opinion explained by the common suppositions.

4. The loss of heat, by a change of media, depends much on the rapidity of the change, for the plastic power of *life*, in varying the process of
animal

animal heat, so as to accommodate it to the external changes, acts for a time with great celerity, though this celerity seems to diminish with the strength.

EXPERIMENT VIII.

I placed in a large room, where the mercury stood at 36° , two slipper baths, at the distance of six yards from each other. One was filled with cold salt water of the temperature of 36° , the other with water heated to 96° , which was my own heat. Undressing myself in an adjoining room by fire, I afterwards slipped on a loose flannel dress, and descended *slowly* into the cold bath, where I remained two minutes; I ascended *slowly* into the air, and then sunk myself in the warm bath, where I remained two minutes also: I returned to the cold bath, where I staid two minutes as before, and removed from it again to the warm bath. But during all these changes of media and temperature, the thermometer, with its bulb under my tongue, never varied from 96° . I attribute this partly to the heat of my body being in some degree defended by the flannel dress, partly to the calmness of the air, but chiefly to the slowness of motion in these changes. It may be said that the time of lying in the different baths was not long enough to

to produce any sensible change in the heat of circulating fluids of such a mass, but this is not consistent with many of the other facts.

5. The influence of the application of cold water to the surface of the body on the heat, is in some respects regulated by the animal vigour, as the following experiment will show.

EXPERIMENT IX.

In the same room I placed a large empty vessel, in which two young men sat down in succession, each with the bulb of a thermometer under his tongue. A man standing on a bench with a bucket of cold salt-water containing four gallons, poured the whole on the head and shoulders, suffering it to run down on the rest of the body. This process took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue sitting without motion for a minute after, during which, in both instances, the mercury rose two degrees. A third, much inferior in vigour, submitted to the same experiment, and the mercury continued during the affusion of the water unchanged, but in a minute after sunk half a degree. In fevers, where the heat is generally increased from two to six degrees above the standard of health, pouring

pouring a bucket full of cold water on the head, always reduces the pulse in frequency, and commonly lowers the heat from two to four or five degrees. Of this salutary practice I hope soon to speak at large to the public*.

6. The power of the body in preserving its heat under the impressions of cold, and the changes of temperature, and of media, seems in some measure regulated by the condition of the mind. That fear increases the influence of cold, and of many other noxious powers, will not be doubted; but the state of the mind to which I allude, is that of *vigorous attention* to other objects. This, it is well known, will to a certain degree deaden, or, indeed, prevent the sensation of cold; and what does this, I apprehend, prevents, or at least weakens, its physical action. The astronomer, intent on the objects of his sublime science, it is said, neither feels, nor is injured by, the damps or the chilliness of the night; and in some species of madness, where the ideas of imagination are too vivid to admit the impressions of sense, cold is resisted to an extraordinary degree. *y* I have seen a young woman, once of the greatest delicacy of frame, struck with madness, lie all

* Written in 1791:

night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice*.

7th. There are particular conditions of the atmosphere, not perfectly understood, that seem to have an influence in depriving us more speedily of our animal heat, than others where the cold is greater.

It may seem, that by this time I had renounced my intention of trying the effects of immersion in fresh water on the animal powers, and particularly on the heat. Some trials, I have, however, made, of which I shall only relate the following.

EXPERIMENT X.

In the same vessel, containing an equal bulk of fresh water, Richard Edwards, the subject of my first experiments, was immersed, at the same hour of the day. His heat previously was 98° , his pulse beat 92 in the minute, the heat in the air was $41\frac{1}{2}^{\circ}$, that of the water 40° . The wind was now in the west, so that in the court where the

* This power of resisting cold is not however general in insanity.

bath stood, there was a perfect calm. As I had some fears of the issue of this experiment, instead of exposing him for a minute naked to the wind before immersion, he was covered with a flannel dress from the air, till the instant he descended into the water, into which he was suffered to sink himself slowly, with the bulb of the thermometer under his tongue. These are important circumstances. The following table exhibits the result.

| | | | |
|--------------------------|--------------------|--------------------|------|
| Immediately on immersion | heat 98° | 14 minutes after | 96½° |
| 1 minute after | 97½ | 15 ————— | 96 |
| 2 minutes | 97 | 16, 17, 18, 19, 20 | 96 |
| 3 ————— | 98 | 21, 22, 23, 24 | . |
| 4 ————— | 97½ | 25 ————— | 95 |
| 5 ————— | 96 | 26 ————— | 94 |
| 6 ————— | 96 | 27 ————— | 93½ |
| 7, 8 | 96 | 28, 29 | 94 |
| 9 ————— | 97 | 30 ————— | 93 |
| 10 ————— | 97 | 31, 32 | 94 |
| 11, 12, 13 . . . | 97 | 33, 34 | 92½ |

He now got out into the air very slowly, and stood in it three minutes, the wind *not* blowing on him. He lost one degree of heat at first, which he recovered. He was then put into a warm bath at 90°, which at first *he felt* warm, and his feet and hands were pained, but in two minutes he fell into a very violent shiver, and his heat fell two degrees. The bath was then heated to 95 and 96°, but still he felt cold. It was heated to 99°: he

continued in it five minutes, and his heat was 91° . The heat was gradually raised to 106° , when the sense of coldness, of which he had complained at the pit of the stomach, gradually went off. Before this I had usually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106° , his own heat was still 93° . He now became sick and very languid, a cold sweat covering his face, his pulse very quick and feeble. He was removed into bed, but passed a feverish night, and next day had wandering pains over his body, with great debility, resembling the beginning stage of a fever. By cordials and rest this went off.

This experiment clearly enough confirms the greater danger of being wet with fresh than salt-water; but in itself points out nothing certain besides, except that it is not to be rashly repeated. I mean to try some of these experiments to a greater extent on the brute creation, when I have procured thermometers better suited to my views. The thermometers I employed had not a sufficient mobility for very nice experiments, and I am well aware, that in particular instances, this may have misled me, though the general results, which is all that is of importance in such experiments, as these, will, I hope, be found just and true.

Before

Before I conclude, I must offer a few observations on the subject that led to these experiments.

1. It is, I think, already well known among seamen, that where there is only the choice of being wet with salt or fresh water, it is always safest to prefer the first. In the heavy showers of rain, hail, or snow, by which gales of wind are generally accompanied, the men that must be exposed to them, ought, like Lieutenant Bligh and his crew, to wring their clothes out of salt-water.

2. In all cases where men are reduced to such distress by shipwreck or otherwise, that they have it only in their power to chuse between keeping the limbs constantly immersed in the sea, or of exposing them to the air while it rains or snows, or of being exposed to it where the sea is at times washing over them, it is safest to prefer a constant immersion; because, in the northern regions, where the cold becomes dangerous to life, the sea is almost always warmer than the air, as the experiments of Sir Charles Douglas shew; and because there is not only a danger from the increased cold produced by evaporation, but also from the loss of heat by the rapid changes of the surrounding medium, as the foregoing experiments point out.

3. Whether, in high and cold winds, without
rain

rain or snow, and where a situation may be chosen beyond the reach of the waves, it is safer to continue in the air, or to seek refuge in the sea, must depend upon several circumstances, and cannot perhaps be certainly determined. The motives for choosing the sea will be stronger in proportion as the wind is high and cold, and in proportion as the shore is bold.

The foregoing narrative shews, that men may survive twenty-three hours immersion in the sea, of the temperature of 38° or 40° * (as great a cold as it almost ever possesses) without food or water, and almost without hope of relief; but that any man ever survived an equally long exposure to the higher degrees of cold of the atmosphere, in the same circumstances, does not appear. Though in the case related, immersion in water did not prevent thirst, yet there is little doubt that it alleviated it, a circumstance of high importance towards the preservation of life.

I have purposely avoided any reasoning on the cause of the loss of vital heat, on the change of media in the experiments recited. It may be supposed that during immersion, the water immedi-

* These numbers ought I think to have been 33° or 35° .
See note, p. 195.

tely in contact with the skin, having become heated to a certain degree, the naked body, on rising from it into the air, was in fact exposed to a colder medium, and thus the loss of heat, in this instance, produced. My examination of the heat of the water during immersion, not having been made in contact with the body, I will not deny, that there is some foundation for the remark; and the cases, it must be allowed, are by no means exactly parallel between immersion in an open vessel, however large, and immersion in the sea, where the constant undulation may be presumed to occasion a continual change in the surrounding fluid. But whatever allowance may be made for the circumstance mentioned, I am persuaded, that the difference between the density of air and water being considered, it is not sufficient to explain the loss of heat in the instance alluded to. The changes of temperature in the living body are governed by laws peculiar to itself. I have found in certain diseases, greater and more sudden variations than any mentioned, from applications of cold, very gentle in degree, and momentary in duration.

In his masterly "Experiments and Observations on Animal Heat," Mr. Hunter has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, because it may be affected by the cold air
in

in breathing. The objection is well founded if the bulb be placed on the upper surface of the tongue, but if it be put under it, and the lips shut, the effects of respiration may be disregarded, as I have found from many hundred experiments. The heat may be observed in this way with ease and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth) some of which have been made for me by Mr. Ramsden, according to a form given, as well as others on Mr. Hunter's plan. From repeated trials it appears to me, that when the usual clothing is on, the heat of the living body may be taken with nearly the same result, and equal certainty, under the tongue with the lips shut, at the axilla with the arm close to the side, and in the hollow between the scrotum and the thigh; every other part of the surface is liable to variation and uncertainty. It is evident, that of these three methods, the first only can be employed (as far as can discover) when the trunk of the body is immersed in water; and even when the naked body is exposed to the cold air, the first method seems the best, the heat remaining most steady under the tongue; the axilla is the next best in order, and the worst the lower part of the groin; for the scrotum, and the parts of generation, lose their heat on the application of cold, more speedily perhaps than

than any other part of the body, the extremities not excepted.

The water employed in the experiments related, contained salt in the proportion of one to twenty-four.

1803. It was my intention to have repeated and enlarged these experiments, but the difficulty attending them, and the pressure of professional engagements, has hitherto prevented me.

The two last chapters have carried us off from the subject of fever, to which we are about to return; it is hoped, that besides illustrating some of the more general effects of cold on the living body, they may assist us in a few observations on the nature of fever, and the mode of operation of the cold affusion, which it is the chief object of this publication to recommend.

CHAP. XVI.

General view of doctrines respecting fever.—Hippocrates—Galen, &c.—The ideas of the author respecting the nature of this disease.

THE great obstruction which men have in all ages experienced in the pursuit of knowledge, has arisen from the promptitude of the human mind to decide in regard to causes. To the weak and ignorant, presumption is as natural, as doubt is intolerable, and with such, belief is almost always a creature of the imagination. Nor do these observations apply to weakness and ignorance only: to retain the mind unprejudiced and undecided, in the investigation of striking and interesting phenomena, till by the painful step of induction, their hidden cause is revealed, is an effort of the most difficult kind, and requires the highest and rarest powers of the understanding. The records of every part of science bear ample

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testimony to this truth, particularly the records of medicine, and in a still more especial manner, that part of medicine which treats of the nature of fever. The most eminent physicians, in every period of the world, impatient of observing and delineating, have been eager to explain, and even to systematize; and the science of life owes its corruptions more to the misapplication of learning, than even to the dreams of superstition. Besides the théories derived from the splendid fictions of the Greek philosophy, various are the false doctrines introduced into medicine in modern times, and from the more certain branches of science. The principles of mechanics, of chemistry, of magnetism, and of electricity, have in succession been employed to explain the motions of life, and have served only to mislead and to deceive.

Hippocrates, who lived at too early a period to be acquainted with the collateral branches of science, studied life and disease in the book of nature, and had the merit of an original observer. Perceiving the increase of heat to be the most remarkable symptom in fever, he assumed this for the cause, and founded his distinctions of fevers, on the different degrees of the intensity of this heat. He had not an instrument that could measure this exactly, and necessarily

trusted to his sensations. In forming his diagnosis, he placed his hands on the breasts of his patients, depending more ~~to~~ the degree of heat, than on the state of the pulse, the nature and connexions of which he did not understand. His practice appears to have been natural and judicious, and founded on his theory. He directed linen, dipped in cold water, to be applied to the hottest parts; drew blood away both by cupping glasses and the lancet; and administered cold water and cooling drinks, particularly barley water and honey. This simple practice will acquit the Coan sage with the candid and reflecting, of many of the idle theories which have been imputed to him, and which doubtless were the offspring of after times.

In the days of Galen, philosophy and science had made some advances, and the corruptions which he introduced into medicine from those sources are well known. Preserving however the doctrine of his master, that heat was the cause of fever, the practice of Galen, founded on this notion, was in some respects bold and successful, as was before observed. To him succeeded the Arabians, who still retained the original doctrine of Hippocrates; but with new modifications and corruptions, by which the real nature of fever was still farther obscured.

In our own country, and in the last century, arose Sydenham. He also was an original observer, and to him medicine is doubtless much indebted. That he recorded symptoms with great accuracy, that he established many important facts as to the treatment of disease, and that he was a more cautious reasoner than his predecessors or his contemporaries, are points that are indisputable. But though he affected not to theorize, he was a theorist in every page of his works, and though he founded his reasonings on what appeared to him self-evident principles, yet time has proved them to be defective and fallacious. It was the postulate of Sydenham, that every disease is nothing else but an endeavour of nature to expel morbid matter of one kind or another, by which her healthy operations are impeded. In this endeavour she is not to be obstructed, but assisted, and the process is to be carefully watched and promoted by which she accomplishes her purpose. By one or other of the emunctories this is finally effected, and till it be effected, health cannot be restored. Under this general notion, the inordinate actions of fever are perpetually compared to the motions of fermentation, by which nature separates the vitiated particles from the blood, previously to their expulsion. The doctrine of Sydenham may be traced to remote ages; under his sanction it spread over Europe,

rope, and has descended in one form or other to our own times. Sydenham's doctrine introduced the system of Boerhaave, (to which we have already alluded) as well as other chemical theories of lesser fame, and the erroneous modes of treatment to which they gave birth. The inordinate heat, which Hippocrates considered as the cause of fever, in the eye of the chemists, appeared naturally enough, a necessary, and within certain limits, a salutary consequence of the process excited by nature to expel the disease !

The dawn of a juster pathology of fever is to be found in the works of Hoffman. Though he also undertook to be the interpreter of nature's intentions, he contemplated her process in fever with more sagacity, and rejecting chemical and mechanical analogies, endeavoured to discover the cause of fever in the peculiar nature of the vital motions. He supposed the noxious cause producing fever (in the language of the schools the remote cause) to operate first on the living solids, occasioning a general spasm of the nervous and fibrous system, beginning in the external parts and proceeding towards the centre. In consequence of this, a contraction of the vessels of the extremities must of course take place, impelling the circulating fluids in an increased ratio on the heart or lungs : which, stimulating these
organ

organs to increased action, the fluids are thereby repelled towards the extremities, and thus the phenomena of fever are produced. There are therefore, according to Hoffman, two distinct sets of motions in fever, the first from the extremities towards the centre, arising immediately from the spasm, and accompanied by a small pulse, anxiety, and oppression; the second, from the centre towards the surface, which is the effort of nature to resolve this spasm, and marked by a full, strong pulse, and increased heat. The first of these sets of motions are baneful, and sometimes fatal; the second are medicinal and salutary. By these views the physician is to be directed in counteracting the morbid actions, and assisting the sanative process of nature.*

Since to theorize seems to be the irresistible propensity of men of genius, this theory of the celebrated Hoffman, which accords with appearances, and has a considerable connection with practice, may be allowed the tribute of applause.

The system of Hoffman, produced that of Cullen, which may perhaps be considered as the prevailing doctrine at present, when, however,

* See Hoffman's Works, vol. i. tom. ii. page 10. *Geneva Edition.*

theory of every kind has lost much of its weight on mature understandings. According to Hoffman, the first effect of the remote cause of fever is the spasm, producing a re-action, as has already been mentioned. Dr. Cullen introduced a previous link into the chain—He contended, that the first effect of the noxious effluvia (the remote cause) was a general debility, affecting the sensorium commune. To this debility he attributed the spasm, and to the spasm the re-action of the heart and arteries; which re-action, continuing till the spasm is resolved, removes the debility and the disease. According to Hoffman, the spasm belongs to the class of motions that he denominates baneful; but Dr. Cullen presumes it to be salutary, and therefore ascribes it in the language of the schools to the *vis medicatrix naturæ*. It is not my purpose to appreciate these changes introduced into the theory of Hoffman, or to attempt to estimate with precision, the Cullenian doctrine of fever, as it arranges phenomena, or applies to practice. Debility of a peculiar kind, spasm, and re-action of the heart and arteries, seem all of them links in the chain of fever, and in the first lines of Dr. Cullen, the history of the disease is recorded with extraordinary minuteness and accuracy. I bow with respect and sensibility to the genius of this illustrious man; but his doctrine

doctrine seems erroneous, in as far as it enters into the supposed intentions of nature, and defective, in passing over the morbid heat, and the morbid association, which form the successive links of the chain.

This doctrine of fever, as well as all the other doctrines of its celebrated author, was assailed by Dr. Brown. Assuming the existence of an unknown principle, as inherent in the living fibres, to which he gave the name of excitability, he explained all the phenomena of life and disease, by means of this principle, acted on by stimuli. These stimuli, applied in the due proportion, produce the just degree of excitement, that is, the state of health. If the stimuli are diminished below the healthy proportion, he supposed the excitability to accumulate; if increased beyond this proportion, he supposed it to be expended; and in these different conditions he attempted to found a general theory of disease. Diseases he divided into two classes, Sthenic and Asthenic, or diseases of increased and of diminished excitement, in the latter of which classes he placed typhus, the fever which we treat. In the excitability, Dr. Brown admitted of no change, except in regard to quantity; in the excitement, no variation, except in regard to strength; and in all universal diseases
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he supposed the whole system to be equally affected.* Having therefore assigned to fever its place in its series of descending excitement, he refused to enquire into its symptoms, or to enlarge on its treatment. It is evident, that such a theory could embrace none of the characteristics of the disease. Debility, the first link in the chain of Dr. Cullen, formed according to Dr. Brown the essence of fever. The existence of spasm he denied, re-action he derided, and the morbid heat and morbid association, he wholly overlooked. It is not however to be disputed, that his general views of disease had a salutary influence on the practice in typhus, by encouraging a more liberal use of wine and opium, remedies of the utmost importance, and now universally adopted.

The various modes of action of the living principle, or to use his peculiar language, of the sensorial power, which were unknown to Dr. Brown, have been observed and illustrated by the author of *Zoonomia*.

In the theory of fever delivered by Dr. Darwin, the influence of morbid association is very fully explained. I am not however disposed to

* This was his general position; it is true he relaxed little from it in his explanations.

adopt this theory, which the learned and candid author himself considers as incomplete, because it assumes the truth of his peculiar doctrines, to which it cannot be expected that a hasty assent should be given—considering their vast extent and importance, and considering also, that many other fabrics of genius of the same kind, have crumbled down into the sand of which they were formed.*

* “What I have thus delivered, I beg to be considered
 “rather as observations and conjectures, than as things ex-
 “plained and demonstrated; to be considered as a foundation
 “and a scaffolding, which may enable future industry to erect
 “a solid and a beautiful edifice, eminent both for its simplicity
 “and utility, as well as for the permanency of its materials.
 “—which may not moulder, like the structures already
 “erected, into the sand out of which they were composed;
 “but which may stand unimpaired like the Newtonian philo-
 “sophy, a rock amid the waste of ages!” *Zoonomia*, vol. ii.
 p. 625. Much as I admire the singular ingenuity of Dr.
 Darwin, the position common to him and Dr. Brown, that
 the sensorial power, or excitability, is a substance which ac-
 cumulates and diminishes in the inverse ratio of the stimula-
 tion, on which his doctrines of fever are founded, has not my
 assent. On the contrary, it appears to me, that whatever the
 nature of the vital energy (the sensorial power or excitability)
 may be, it does not act in a way that admits the relations of
 quantity to be applied to it, and the contrary doctrine, which
 introduces into the system of Dr. Darwin so many epicycles,
 seems to me the mortal part of his work. I am aware that
 Dr. Darwin holds the doctrine mentioned with certain modifi-
 cations; which however do not appear to me satisfactory.

Dismissing

Dismissing therefore, as far as it is possible, all theories from the mind, let us briefly consider the process of nature in fever, and examine what inferences may be safely drawn from the succession and catenation of the leading symptoms, and the remedies which experience has pointed out as most successful. Our observations must of course be considered as applying to the typhus, or low contagious fever of Britain, more particularly, but generally to all idiopathic fevers; and if they should be found as little satisfactory in their result, as the conclusions of others, the learned and candid reader will not, it is hoped, suffer his opinion of the accuracy of the author's facts to be affected by the fallacy of his reasonings, but extend his forgiveness to one other abortive attempt to illustrate the nature of fever, an important, but difficult and long contested subject.

Medical science has not ascertained the various remote causes which may produce fever, and I avoid this inquiry, on which I have nothing new, that is satisfactory, to offer.* The first

* In confining the remote causes of fever to contagious human effluvia, and to marsh miasmata, Dr. Cullen, probably simplified too far. Cold, under certain circumstances, seems to produce fever, and the constitution seems at times to fall into fever from unknown and internal causes.

symptom of the disease is a debility or languor, very distinctly expressed on the countenance, followed by paleness, sense of cold, and tremor; the functions of the mind are weakened in a correspondent degree with those of the body, and all the actions of the system seem enfeebled. This general debility is peculiar in its kind. The symptoms resemble those produced by inhaling certain gases, whose properties are known; and this circumstance gives some support to those notions of the nature of contagion, which the new chemistry has suggested. They seem also to have a resemblance, though a more remote one, to the effects of certain poisons operating on the stomach, or introduced into the circulation by the absorbents. The remote cause of fever itself, may perhaps be considered as a poison, acting directly on the sensorium commune. Where this poison is concentrated very much, and highly malignant, or where the system is much debilitated, the powers of life are sometimes oppressed and extinguished in the first stage of the disease. In general however, a re-action or resistance commences; the heart and lungs are roused into increased exertion by the pressure of the accumulating fluids, and repel them back on the surface and extremities; while a spasmodic, or morbid stricture of the extreme vessels, opposes the reflux of the fluids, and thus maintaining the inordinate pressure on the

centre,

centre, excites the heart, arteries, and lungs, to still more violent exertions. In this contest the stomach is usually brought into sympathy, and nausea and vomiting are often induced.

This constitutes what appears to be a struggle between the living energy, and the morbid cause—between the power of the centre, and the resistance of the extreme vessels—in the course of which, a degree of preternatural heat is generated, and the phenomena of the hot stage are produced. When the powers of life prevail, the stricture on the extreme vessels, and on the exhalants of the skin, at length gives way, and a profuse perspiration being poured out on the surface, the heat is carried off, and the febrile symptoms subside. Without entering into detail, this may be considered as a general view of the single paroxysm. It often however occurs, that the solution of the stricture on the extreme vessels does not take place, or is incomplete, and that the morbid heat is not carried off. In this case the morbid actions go on, and the disease runs into continued fever. For some time the stricture on the extreme vessels remains, and the heat is preternaturally great; but these symptoms do not continue through the whole course of the disease. On the contrary, the heat towards the latter stages, will sometimes be found as low as the natural standard, sometimes lower, and

and the capillaries of the skin be completely relaxed; while the inordinate action of the heart and arteries continues, being carried on, as it seems to me, by the morbid association produced in the course of the disease, which retains its influence in this, as in other cases, after the cause that produced it, ceases to operate.

To this general view of fever, it is probable that little objection will be offered, by those who are familiar with the disease. But exceptions may be taken to the language, as not sufficiently clear of theory, against which the author had declared. It may be said, for instance, that in applying the active forms of speech to the re-action or resistance of the constitution, we seem to run into the Stahlian doctrine, or at least to assume the existence of the *vis medicatrix naturæ* of Dr. Cullen; and that to enumerate spasm as a link in the chain of fever, is in fact to admit the leading peculiarity of his system. To this it may be replied, that there is no subject on which the imperfection of language is so much perceived, as in our attempts to describe the phenomena of life. In the strictness of speech we properly employ the passive mood of our verbs, in recording the phenomena of inanimate matter, and confine the active mood to those vital motions, which are accompanied with consciousness or design; but there are various
actions

actions peculiar to life which are not accompanied by consciousness, or subject to the will, and which depend on laws wholly distinct from those which regulate inanimate matter. If we borrow the phraseology and the *mood* in which we speak of inanimate matter in recording these actions, we are apt not only to form indistinct conceptions of their nature, but to apply the principles which regulate inanimate motions to the motions of life. This is an obvious, and as experience teaches us, an ample source of error. If on the other hand, we employ the active forms of speech, these immediately suggest consciousness and design, and the imagination forms to itself an ideal being, as directing these actions, in whom consciousness and design may reside. This also is a natural and fruitful source of error.—Both these extremes we would avoid. But the penury of language obliges us to use one or the other mood in treating of the phenomena in question; and on reflection it appears, that in our present ignorance of the first principles of life, a clearer notion will be formed of the nature of those motions in which the whole system sympathizes, destitute though they be of consciousness and design, by the occasional use of the active forms of speech, since they have a much stricter analogy with those vital actions, to which these forms are justly applicable, than with the motions of inanimate matter. Considerations of this

kind

kind may have influenced the late John Hunter in the use of his terms, which are chiefly *active*, and which, though far from perfect in themselves, are more happy than those of former physiologists, as his doctrines of life were more original, more unprejudiced, and in general more accurate and profound.

Debility of a peculiar kind, is then the first operation of the remote cause producing fever—the necessary consequence, or as some contend, the concomitant effect, is a spasm, or contraction of the arteries; but more especially of the extreme vessels, and the capillaries of the surface—hence follows an accumulation of blood on the heart and lungs—the re-action of those organs—the generation of morbid heat—and of morbid association. On each of these links in the chain of fever, let us offer a few remarks.

1. It has been asserted by some inveterate theorists, that the debility in fever has nothing in it peculiar. It is enough to reply, that simple debility often exists even in extreme degrees, without producing the phenomena that constitute fever.

2. It has been asserted, that a spasm on the extreme vessels does not exist in fever, or that if it

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does

does exist, it may safely be disregarded. Those who hold the first opinion, suppose the paleness and shrinking of the surface to arise merely from the enfeebled circulation, by which the blood is not propelled into the extreme parts; and doubtless this has its effect. In syncope, however, where this paleness and shrinking arise from pure debility, as soon as the heart and arteries recover their energy, the blood flows immediately back into the extreme vessels as before. That this is not the case in fever, the most cursory observation will convince us. After the re-action of the heart and arteries has been some time established, the blood is indeed propelled through the vessels of the surface that circulate the undivided fluid; but the capillary arteries that separate the aqueous part, remain constricted, as well as the orifices of the skin that pour it out on the surface.

3. It is however contended, that this stricture may be disregarded, since it is now very generally admitted, that the perspirable matter is not excrementitious, and since we find that its obstruction in other circumstances, and from other causes, produces little or no injury to the constitution. A little reflection will serve to refute this position. Spasm, or morbid stricture on the extreme vessels, must necessarily affect the condition of the heart and lungs, from the increased pressure

pressure of the circulating fluids on these organs : it must also affect the stomach, not only from the connexion of that organ with the heart and lungs, but from its direct sympathy with the surface.—Dr. Cullen has put these points beyond controversy. But this is not all ; the obstruction of perspiration—of profuse perspiration—in the paroxysm of fever, obstructs the process by which the constitution expels the morbid heat, and thus leaves the system under the influence of a general stimulus of the most powerful nature. It was a position of the celebrated Boerhaave, that the morbid heat in fever, being a symptom only, might therefore be disregarded.—But can we suppose, that a heat, six or seven degrees greater than that of the blood in health, however generated, will not have the most important effects on the system, and if it stands in the relation of effect to the preceding symptoms, that it will not operate as a cause on those which succeed ?—Doubtless this morbid heat e-acts on the vascular system ; irritates the spasm of the extreme vessels ; and prolonging the increased action of the heart and arteries, establishes a morbid association, which carries on this increased action after the spasm has relaxed, and the heat self subsided.

4. What then, it may be inquired, is the actual state of the animal heat in the different stages

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of

of the paroxysm of intermittent, as measured by the thermometer? In different trials it seemed liable to considerable variety, but the following is the general result, which however the reader will receive, as subject to the corrections that a more extensive experience may suggest. In the cold stage, the heat is diminished, not on the surface only, as some have imagined, but very probably over the whole system. I have found it under the tongue, and at the axilla, as low as 94° , 93° , and 92° ; and on the extremities many degrees lower. At this time the stomach feels cold, and universal tremor takes place. Warm, and even hot liquids, are highly grateful, and the warm bath, heated to 100° , more grateful still. The drinking of warm liquids should be freely indulged in, and there is reason to believe, that immersion in the warm bath, as practised by Galen, would be found as salutary as it is grateful. By degrees the pulse increases in frequency and force, and the heat begins to rise, but with much irregularity. It doubtless accumulates first in the centre of the system, and from the centre is pushed along with the blood, towards the surface. This process does not however go on in any regular progression, but like almost all the other animal processes, with alternate increase and relaxation. At times it appears retrograde and even when it is so far advanced that the heat taken at the axilla, and under the tongue, is greater than

than the standard of health, a slight accession of external cold will produce a general chilliness, and bring back the oppression on the heart and lungs. On the surface itself the restoration of heat takes place with no regularity. Certain parts are heated first. In certain parts the heat is above what is natural, while in others, it remains below this standard; and hence arises that mixed sensation of heat and cold, which every one acquainted with fever must have experienced in the transition from the cold to the hot stage of the paroxysm.

The irregularity with which the heat of the surface is restored, probably arises from this circumstance, that two causes conjoin in producing this effect—the reflux of warm blood from the centre, and the sympathy between the stomach and skin. To this last is attributed the circumscribed heat of certain parts of the surface, while the rest remains cool, this local heat being probably excited by the increased action of the vessels of these parts, awakened by the restored heat and tone of the stomach, which in other circumstances produce similar effects. In general the sense of cold predominates even after a morbid heat has taken place at the axilla, under the tongue, and in different parts of the abdomen and thorax; because a considerable part of the surface, and the whole of the extremities, still remain below the natural temperature, and the sensation

sensation which this produces, arising from parts of great sensibility, overcomes the sensation of heat from the deeper seated parts, whose sensibility is so much inferior. At length however the heat of the surface becomes general and uniform, rising to 102° , 103° , 104° , and sometimes 105° of Fahrenheit. I have not seen it higher than this last degree in the paroxysm of intermittent, and seldom in continued fever; though different authors speak of febrile heat four, or even five degrees higher. The sensation of heat is now strong and steady, and the accession of external air does not produce a chilliness as before.—This is the time for the affusion of cold water, as has already been mentioned. The sensation of heat is most powerful on the extremities, particularly on the palms of the hands and the soles of the feet.—This arises in some degree from the great sensibility of these parts, but in some degree also from this circumstance, that in their natural state they are two or three degrees cooler than the trunk of the body, and cannot be raised to the general temperature of health without an uneasy sense of heat, which is frequently relieved by a partial perspiration. When raised to the general fever heat, their temperature is therefore increased in more than a proportional degree beyond its usual standard.

After

After remaining some time in this hot stage, the skin begins to relax, and to become softer and smoother to the touch; insensibly a moisture takes place, terminating generally in a profuse sweat.—Neither however does this process go on regularly; after the moisture commences, it sometimes goes off, and a dry burning state of the skin returns: again the sweat commences, and when it becomes general, and continues steady, it is critical, that is, it terminates the paroxysm. As it commences, the heat declines, though at first very slowly; as it proceeds, the heat subsides gradually, and when it terminates, the temperature of health is restored. The sinking of the morbid heat is however a good deal regulated by the quantity of the bed-clothes, and the closeness with which the body is enveloped.—Where the whole covering that was laid on during this cold stage is kept on during the hot stage, (as often happens, through a mistaken wish to force the sensible perspiration) the heat is carried off with difficulty, and it requires the perspiration to be very profuse to reduce it to its natural standard. On the contrary, if the clothes be lessened after the hot stage is fairly established, the heat passes off with less difficulty, and the perspiration requisite to remove it is much less profuse.

These details will not be thought superfluous or
tedious

tedious by those who consider, that after all that has been written on the subject of fever, thermometrical observations on the rise and progress of febrile heat are in a great measure new.*

5. Whether it be owing to the nature, or force of the contagion, to the state of the constitution, or to some other unknown cause, the re-action of the system does not always resolve the spasm or morbid stricture on the extreme vessels. When this stricture is not resolved, the sweat is not effused, the morbid heat is not carried off, and the diseased actions run into continued fever,

If a person is confined in a hot room, or in the hot bath, till his heat rises three or four degrees above the natural standard, his pulse will be found of a feverish rapidity; wandering pains will soon be felt over the body; languor, lassitude, and at length great debility will take place, with most of the symptoms of regular fever. It is evident that these symptoms cannot be expected to go off till the inordinate heat is removed, and if the person remain some time in the heated medium, he will

* I intended to have introduced here one or two registers of the heat and pulse, taken every half hour during the paroxysm of intermittent, but this is delayed till I am enabled to speak from more numerous observations.

find, that the inordinate action of the heart and arteries continues after leaving it, and even after his own heat has subsided to its natural standard. This depends on a principle peculiar to life, to which the name of habit, or association, has been given, and which extends its influence to all the vital phenomena, whether intellectual or corporeal.

In fever this morbid heat does not arise from the surrounding medium, but from certain motions in the system itself. But however generated, a heat five or six degrees above the natural standard must be a powerful agent, and it cannot be expected that the diseased actions should subside under so strong a stimulus. The rapidity of the circulation, and the labour of respiration, are consequences of a heat of this degree from whatever cause arising, and must continue till the heat is reduced.

We may here observe, that it is not by any means easy to make an accurate comparison between the state of the system under the heat of fever, and under an equal degree of heat introduced from the surrounding medium, because it is in fact very difficult to raise the heat of the body in a state of health to the highest degree of fever heat, either by heated air or heated water.

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The experiments recorded by Sir Charles Blagden prove this in respect to a heated atmosphere; the heat, as it is accumulated in the system, being dissipated by profuse perspiration, or by some other living process, of which perspiration is a concomitant effect. I have found the same difficulty in heating the body in the hot bath, for there also the heat is with difficulty increased beyond 100° or 101° , when sweat, as is usual, breaks out over the surface. Even in these degrees the greater part of the symptoms of fever will be felt. The heat however is moderated, and, when the temperature of the external medium is reduced, it is carried off by the discharge from the surface. But in fever this is prevented, for the peculiar debility induced by the remote cause, occasions, or is attended by, a spasm, or morbid stricture of the capillaries of the surface and of the skin itself, by which the insensible perspiration is prevented from increasing in proportion to the heat, and the sensible perspiration obstructed. Hence the difficulty of reducing the heat in fever; and thus it appears, that the spasm on the extreme vessels, which some of our latest physiologists have wholly neglected, is perhaps the most important peculiarity of the disease.

It may be observed, and it furnishes a subject of interesting inquiry, that this stricture, which
often

often gives way under a moderate re-action, maintains itself with rigidity where the re-action is great. Thus, where the heat rises to 99° , 100° , or 101° , the orifices of the skin often relax, and carry it off by increased perspiration; while at 104° and 105° , with a proportionable force of circulation, they are in general obstinately constricted. This is very striking in fever, but not peculiar to it. Even in the state of health, while a moderate, gradual, and general stimulus from heat opens the pores of the skin, a sudden and considerable increase of heat constricts them; the violence of the stimulus, in this, as in other instances, occasioning a morbid resistance of the living fibre on which it acts. This resistance to inordinate stimuli extends to every part of the living system, and belongs to every species of vital action—it may be considered as peculiar to life.

But, it may be justly remarked, that after fever is established, the reduction of heat to the natural standard, however desirable, is attended with difficulty and hazard, since the patient cannot in general bear the continual exposure to external cold necessary for this purpose.

Even in the hot stage of fever, if the patient be exposed naked to a cold atmosphere, though he supports the application for some time with comfort,

fort, and even with advantage, yet, as the heat of the surface approaches the standard of health, a sudden sense of chilliness comes on, with a return of oppression on the heart and lungs, and all the symptoms of the first stage of the paroxysm. The length of application, and the degree of external cold producing this effect, are indeed various in various constitutions, and in different stages of the disease: the degree of reduction of the animal heat at which the chilliness commences, is also uncertain; but the general truth of the position laid down is established on ample experience, and will not, I believe, be controverted. In cooling the surrounding atmosphere in fever, or in any other continued application of cold, care must be taken to keep it within the limits in which it is grateful to the sensations; and within these limits the cool treatment, as far as my observation extends, is uniformly advantageous. But though it moderates re-action, it does not in general remove the spasm on the capillaries, or break the association or habit by which fever is prolonged.

This is effected by the sudden affusion of cold water over the naked body; and the mode of its operation will now be easily understood. The sudden, general, and powerful stimulus given to the system, dissolves the spasm on the extreme vessels of the surface, and of the various cavities of the
body;

body; the sudden and general evaporation carries off a large portion of the morbid heat accumulated under the skin, and the healthy action of the capillaries and exhalants being restored, the remaining superfluous heat passes off by sensible and insensible perspiration. The stimulus of morbid heat and of morbid stricture being removed, the morbid association seems also broken by the sudden and powerful impression on the sensations—in fact, the inordinate action of the heart and arteries subsides, and the harassed and toil-worn patient sinks into that peaceful sleep which nature has provided as the solace of our pains and sorrows, and the restorer of our strength.

If such be the explanation of the diminution of the heat, and of the frequency of the pulse, which follow the affusion of cold water on the surface, it may here be enquired how the diminution of the thirst is accounted for. Are we to suppose, that a portion of the liquid is taken up by the absorbents of the surface, as is generally believed to be the case, during immersion in the bath? I apprehend not. The absorption in the bath, if it occur at all, of which there is no sufficient evidence, has been greatly exaggerated: and the diminution of thirst, after the cold affusion, is too speedy to be ascribed to such a cause. It admits of a more satisfactory explanation. The morbid stricture on
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the extreme vessels, to which we impute so much, is not confined to the capillaries of the surface, but extends, as far as we have an opportunity of observing, to the capillaries of all the cavities of the body. The sense of thirst is produced by the heat and dryness arising from the morbid stricture of the exhalants in the inner surface of the mouth, fauces, and stomach; and this stricture, being removed by the affusion on the surface of the body, from the sympathy between the surface and these cavities so often alluded to, the healthy action of the exhalants of these parts, as well as of those of the skin, is restored; and their moisture being poured out, the heat and dryness are removed, and the thirst diminished.

This indeed seems to be the way in which thirst is relieved in the first instance, by liquids taken into the stomach itself, where the effect is too sudden to arise from the absorption of any portion of the liquid into the mass of circulating fluids. We cannot indeed perceive this effect on the exhalants of the stomach, but we may reasonably suppose it to take place where we observe the swallowing of drink to be followed by a relaxation of the exhalants on the skin, which must proceed from their sympathy with those of the stomach previously relaxed; and it may illustrate and support this position, that when liquids, swallowed even in large quantities

quantities in the hot stage, do not produce a sensible increase of perspiration on the skin, the relief they afford to the thirst is momentary only. In such cases we presume the stricture on the exhalants of the stomach has not been resolved. The relief from thirst afforded by liquids taken into the stomach is in general however more effectual and complete than from affusion of water on the surface, because, when the stricture on the capillaries is removed, a rapid absorption takes place from the stomach, and the circulating fluids are diluted. But on the surface of the body this absorption is doubtless slow and imperfect, and it will afterwards be shewn, that there is reason to doubt whether it takes place at all. It was remarked that the affusion of tepid water on the surface, though effectual in diminishing heat, (page 71) is not followed by the entire cessation of fever, as is often the case after the cold affusion. The cause of this is obvious—the coolness produced by the tepid affusion arises from the increased evaporation, and continues only while this continues. The stimulus given by the tepid affusion is comparatively slight and transient—it does not resolve the spasm on the surface, or on the other cavities of the body, and it does not destroy the morbid catenation. The heat therefore speedily returns, when the remedy is withdrawn.

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This explanation of the operation of the affusion of cold water seems to me satisfactory. The cases which have been related shew, however, that it is only in the early stages of fever that it produces a solution of the disease. In the after stages, though uniformly advantageous while the morbid heat continues, its effects are not so decisive, the morbid actions having acquired the force of habit, or in other words being carried on by the associations that have been produced.

What then ought to be the indications of practice in fever? To diminish the cold in the cold stage; to moderate the heat in the hot stage, and to resolve the stricture on the extreme vessels by which the morbid heat is retained, and the reaction prolonged; and where the inordinate action of the vascular system continues after these objects have been attained, to support the powers of life till the morbid associations, or habits of action gradually die away, from the removal of the cause by which they were introduced. In addition to these general indications, it will be essential to secure the proper action of the bowels, and in every case to unload the alimentary canal of its morbid contents, whether these contents have become diseased through the action of general fever, or as there is reason to believe, in some of the fevers

of the warm climates, be the remote cause by which fever is produced.

If these general observations should excite attention, they will doubtless excite objections also, and perhaps animadversions; but whatever be the fate of his reasonings, the author rests with some confidence on the stability of the facts by which they have been suggested. In the view which he has taken of fever, the supposed intentions of nature are not inquired into; and of course no attempt is made to arrange the symptoms as they arise from the direct agency of the febrile poison, or the exertion of *her* medicating power. Such attempts are as unnecessary as they are difficult. It is not indeed to be denied, that the living system, after being excited into morbid action, passes frequently unassisted, through a succession of symptoms into a state of health, differing in this in every other respect, from any of the arrangements of inanimate matter. But it is equally true, that it often sinks in the course of this process, sometimes destroyed in fever by the excess of its own re-action, and sometimes by the habits of action or associations produced in the course of the disease.

It is a serious error to suppose that the febrile poison, if so we may call it, being received into
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the system, is the principal cause of the symptoms, and that they consist of a struggle of nature to expel it, without which health cannot be restored. It is safer to consider it as an agent that excites the system into fever, which however is carried on, not by the continued presence and agency of this agent, but by the principles which regulate the actions of life. We are not therefore to wait for the sanative process by which nature is supposed to separate this *virus*, and to throw it off, watching her motions, and assisting her purposes; but to oppose the fever in every stage of its progress with all our skill, and to bring it to as speedy a termination as is in our power. When we dispel the morbid heat, and reduce morbid re-action in the hot stage of the original paroxysm, by the powerful means of the cold bath affusion, the whole of the febrile symptoms vanish; a sufficient proof, that in this stage of the disease, these symptoms arose from inordinate heat and inordinate action, and not from a poison circulating with the blood.—Hence the safety and the wisdom of decisive measures in the earlier stages, before the strength is materially impaired, or the diseased habits established. Those who practise within the tropics, where fever runs its dreadful course with such rapidity, ought especially to be aware of this truth, with which indeed the most intelligent of their number seem now to be impressed. In those climates

ates, however, the disease ought to be combated, not merely by affusion of cold water on the surface, but by immersion of the patient in the cold bath. In this fever, hours, nay minutes are precious, and as the cold stage is short, the application of this remedy admits little delay. As soon as the second, or hot stage is formed, a judicious and resolute application of cold might supersede all other remedies. But the temperature of the sea, and even of the springs within the tropics, rising generally as high as 77° or 78° of Fahrenheit, affords a stimulus to the sensations from simple affusion, of a weaker kind, and hardly permits us to hope, that in the form of affusion, the cold bath can be followed by those effects in the ardent fever of the torrid zone, which in the milder fever of this climate it so happily produces. Artificial modes of cooling the water may indeed be resorted to, and when this can be effected, the repeated use of the affusion may in all probability extinguish the disease. But in military duty, as well as in most other situations, artificial methods of cooling a sufficient quantity of water it may often be impossible to employ. In such cases, when the physician has ascertained that the heat of his patient has risen to the degree which justifies the practice, let him plunge him fearlessly into the cold bath, or the sea. The duration of immersion must depend on the effects on the pulse, on the sensations, and on

the heat, measured by the thermometer; and a greater degree of coolness will be produced by alternately raising him into the air, where the wind blows over his naked body, and sinking him into the water, than by continued immersion. The utmost care is necessary in a process of this kind, to guard against the effects of fatigue. Various instances might be adduced of the success of this practice in persons who have, in the delirium of fever on shipboard, plunged into the sea. I have received a variety of authentic relations of this nature. These have happened on board of our ships at sea, during the delirium of fever, and chiefly in the warm climates; and in every instance where the patient has been fortunately taken up, recovery has followed.

An accident of this kind occurred about three years ago to Captain S——, of this port, in the Irish channel. He sprang out of his cabin window in the height of delirium, and was upwards of twenty minutes in the water. He was taken up perfectly calm, and speedily recovered.—If it be considered, that in the Irish channel, even in summer, the temperature of the water is seventeen or eighteen degrees colder than in the West Indies, the importance of this fact will be clearly perceived. But while the different modes of applying cold water to the surface are employed, it
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ought also to be poured into the stomach in large quantities, when the patient's heat will permit it, and the presence of nausea and vomiting is no objection to this practice, if a chilliness of the stomach is not produced.

When we consider the general adoption of cold drink in the ardent fever of the ancients, its success in the Hungarian fever, to which the yellow fever bears such analogy, and the pointed recommendation it has received from Hoffinan in bilious vomitings and dysentery, it appears rather extraordinary that it has not hitherto been resorted to in the diseases of the West Indies.

The salutary effects of the cold bath, and of cold drink in fever, strongly recommend the adoption of these remedies in the plague. On the northern and eastern shores of the Mediterranean, where the liberal use of cold drink in fevers prevailed throughout antiquity, and on some parts which it seems to have been carried to a great extent in the beginning of the present century, there is reason to believe that it has now fallen into disuse. If we give credit to the answers made to the queries of Mr. Howard in regard to the treatment of the plague, the administration of cold drink forms no part of the regular practice in this terrible disease. The Jew physician of Smyrna

na indeed says, "that the Turks, in the violence
 " of the fever, take handfull of snow and apply it
 " over their bodies, and also eat it; and likewise
 " sometimes throw cold water on their feet." But
 the learned Hebrew cannot determine whether it
 is of service, "as these people in other respects
 " pay no regard to the rules of diet." *Howard*
on Lazarettos, page 39. Morandi, physician at
 Venice, observes, "that some sailors at Constan-
 " tinople, in the phrenzy of the plague, have
 " thrown themselves into the sea, and it is said,
 " that on being taken out, they have recovered."
 But this happy temerity the regular practitioners
 do not appear to have imitated, a fact that is fur-
 ther established by Dr. Russel, in his account of
 the treatment of the plague at Aleppo.*

The administration of cold drink in the plague,
 as well as the external application of cold water,
 must however be regulated by the actual state of
 the patient's heat, and of his sensations of heat,

* When Mr. Howard was printing his work on *Lazarettos*
 at Warrington, in 1788, I communicated to him an account
 of the successes of our first trials of the affusion of cold water
 in fever, in the Liverpool Infirmary, and he promised to make
 more particular inquiries in regard to the empirical use of
 this remedy, in his ensuing journey to the East, in the early
 part of which, it is well known, his career of benevolence
 terminated.

of which also we have to lament that we have no accurate information. If it should be found that in this malignant disease, the heat is little or not at all above the natural standard, (as is the case in the advanced stages of the confluent small-pox) our hopes from the exhibition of this powerful remedy would probably be disappointed. But the trial is recommended by every consideration.

CHAP. XVII.

Animal heat—its origin.—Perspiration—its cooling influence.—Mode of operation of other remedies in Fever—Antimonials—Opium, &c.

I HAVE elsewhere observed, that if a definition of life were required, it might be most clearly established on that capacity, by which the animal preserves its proper heat under the various degrees of temperature of the medium in which it lives. The more perfect animals possess this power in a superior degree, and to the exercise of their vital functions this is necessary. The inferior animals have it in a lower degree, in a degree however suited to their functions. In vegetables it seems to exist, but in a degree still lower, according to their more limited powers, and humbler destination. As the capacity of preserving nearly an uniform temperature in all the varieties of climate and season, is a criterion of life in the more perfect animals, so among individuals of the same species,

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the degree of this capacity may be considered as a criterion of the strength of the living principle.

There is reason to believe, that while the actual temperature of the human body remains unchanged, its health is not permanently interrupted by *by* the variation in the temperature of the medium that surrounds it; but that a few degrees of increase or diminution of the heat of the system, produces diseases and death. A knowledge therefore of the laws which regulate the vital heat, seems to be the most important branch of physiology.

Modern chemistry assumes to itself the discovery of the origin of animal heat, which is supposed to depend on those changes in the ingesta, which the functions of life are always producing. Among the breathing animals, the principal of these changes is occasioned by the lungs on the atmospheric air which they inhale, and respiration is considered as the principal source of the heat of this class of animals. That the oxygen contained in the atmosphere furnishes the largest portion of the pabulum of life, is now scarcely doubted, respiration being a process in which it passes from its gaseous to its concrete state, giving out a portion of the heat it held in vapour, in the same manner steam in its conversion into water, gives out a

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portion

portion of its heat. The heat thus extricated, being conveyed by the circulation to all the parts of the body, is the principal means by which the whole is warmed and animated*.

Some physiologists of our own country, and on the continent, have of late supposed, that a similar process is carried on by the surface of the body, through which also, though in an inferior degree, the oxygen of the atmosphere is inhaled. Experiments are wanting to establish this position, and there

* Respiration is a case of compound elective attraction, in which the oxygen gas is decomposed, and enters into new compounds; a part seems to be absorbed by the general mass of blood circulating through the lungs; a part unites with the hydrogen of the venous blood, and forms water; another part with the carbon of this blood, and forms carbonic acid gas. In the two first cases the language in the text applies strictly, but in the last case the heat extricated does not arise from the oxygen becoming concrete, but from its entering into the composition of a gas which has a less capacity for caloric than itself. The heat disengaged in these different processes unites with the blood, which in losing its hydrogen and carbon (hydro-carbonat) is converted from venous to arterial, and has its capacity for caloric increased. Thus inflammation in the lungs is prevented. But the arterial blood, in the course of circulation, again absorbs by little and little a portion of hydro-carbonat, parting with caloric in the same proportion. Hence the uniformity of temperature throughout the body. This seems the modern theory of respiration, which it must be confessed is not without its difficulties.

there is reason to believe that the principal function of the skin, in the breathing animals at least, is of a different and opposite nature. That an animal possesses to a certain extent the faculty of rendering sensible heat latent, or to speak more philosophically, of reducing caloric from a free to a combined state, in cases in which the stimulus of heat might otherwise overpower the living energy, there is reason to believe, from a variety of experiments and observations. And that this is in part performed by the perspiration from the surface, can scarcely admit of a doubt. The process of perspiration which is continually going on from every part of the body, is in this point of view the converse of that of respiration: as in respiration a gas is constantly converted into a solid or fluid, and thus heat evolved, so in perspiration a fluid is continually converted into a vapour, and thus heat is absorbed. If then we suppose, that while the proportion of oxygen received into the system continues the same, the temperature of the atmosphere is increased, we can understand why our heat is not increased by supposing an increase of perspiration. And if the temperature of the atmosphere remains unchanged, while the oxygen received by the lungs is increased, we can still explain the stability of our heat, by supposing an increase of perspiration. The first of these suppositions is nearly realized, when a warm day comes after cold weather; the

the second is realized, when an increased respiration takes place under exercise—thus perspiration appears to have a principal share in regulating the animal heat, and the chain of life seems connected with the physical world by two links, which the recent discoveries in chemistry enable us to unveil*.

A vessel

* I have said *nearly* realized, in the beginning of the sentence. Supposing the same bulk of air breathed in warm as in cold weather, the proportion of oxygen will be rather less in the first case, from the diminished density of the air. There is reason to believe also, that an animal contaminates the air more slowly in warm weather, that is, receives a less proportion of the oxygen it inhales, into the system. But these particulars, in the rapid and general views which I offer, are necessarily overlooked.

Dr. Mitchel, of New York, in his paper on the nature of contagion, has asserted, that the actual heat of the animal is always proportioned to the quantity of oxygen inhaled; but a very cursory examination of facts will shew that this is erroneous. By the experiments in Chap. XV. (and various others might be adduced) it will be seen, that the living body has the power of preserving its heat nearly unchanged for a considerable length of time in so dense a medium as water 50 degrees colder than the blood. This could not arise from an increased inhalation of oxygen. The phenomenon is partly to be explained from the sudden contraction of the perspiratory organs; but principally, from the powerful impression on the sensations, rousing the vital principle to increased exertion, and

A vessel filled with water, and exposed to the atmosphere cannot be raised above 212° of Fah^t. by any quantity of fuel, because, as heat is applied from below, evaporation carries it off from the surface. Hot springs are of the same heat at their fountains throughout the year, probably because the evaporation from the surface of their waters, increases and diminishes with the heat of the atmosphere. In like manner we may suppose the heat of the living body to be kept uniform by the evaporation from its surface increasing or diminishing according to the quantity of heat extricated in the system, or received from the surrounding medium. But the cautious reasoner, aware of the deceptions arising from such analogies, will here very properly inquire—Does the perspiration by the skin, the body remaining at the temperature of health, actually go on more rapidly in warm than in cold weather? Is it greater under exercise than when at rest? Is it more plentiful when oxygen is received in abundance into the system, than when it is imbibed more sparingly? We run little hazard in answering the two first of these questions in the affirmative, but in regard to the last, farther

and to increased extrication of heat. It is to be observed too, that it was the trunk, and more solid parts of the body that reserved their heat, in the experiments referred to—the extremities became cold.

experiments

experiments are requisite to enable us to decide.

The insensible perspiration attracted the notice of physicians very early, and it was a common dogma among the ancients, which has descended with too little examination to our own times, that the whole surface of the body inhales and exhales. The subject has been considered more accurately since the experiments of Sanctorius in the middle of the last century; and the great quantity of matter supposed by him to pass off by this process, pointing out its importance in the animal œconomy, theories of disease, founded on the diminution or obstruction of this discharge, have since his time prevailed universally. The quantity of the matter of perspiration is not easily ascertained, and this uncertainty must continue while the effect of respiration on the weight of the body remains undetermined. It was for some time believed, that the quantity of perspiration from the surface had been exaggerated by Sanctorius; but since a notion has been entertained of the great absorption by the skin, his calculations have been supposed to fall short of the truth. If however we calculate that fifty ounces of water are converted into vapour on the surface of the body in twenty-four hours, this must produce a very considerable absorption of heat, and have a powerful influence on the

temperature of the living system; and however we may distrust the experiments hitherto made, on the quantity of the matter of perspiration, there is no reason to question the points in which they all agree—that this discharge is greater in warm weather than in cold—under exercise than when at rest*.

Important

* Very various calculations have been made of the quantity of matter perspired. The only experiments made in this country on this point, that deserve much notice, are those of Dr. Cruikshanks. He appears to have lost upwards of seven pounds weight in this way in twenty-four hours, when at rest, in an atmosphere of 71° ; but under exercise a much larger quantity. This exceeds the calculation of Sanctorius.

1803. In the report of M. Lavoisier and M. Seguin, on transpiration, (perspiration) published in the last volume of the Memoirs of the Academy of Sciences, printed in 1797, this is stated differently: According to their experiments, the loss of weight by perspiration and respiration which an individual sustains in a moderate heat, and who does not undergo much bodily exercise, varies from 11 to 32 grains in the minute, that is, from one pound eleven ounces and four drachms, to five pounds in twenty-four hours. The medium may be taken at two pounds thirteen ounces in twenty-four hours, of which one pound fourteen ounces passes off by cutaneous perspiration, five ounces five drachms and sixty-two grains by pulmonary perspiration, and three ounces three drachms and ten grains, by the consumption of carbon in respiration. The sum of this loss is however greatly increased by exercise, or an increase in the heat of the atmosphere. It is needless to add, that the experiments of Lavoisier and Seguin.

Important as the evaporation from the surface of the body must be in regulating the heat, it is remarkable how little it has hitherto been considered in this point of view. In the year 1755, Dr. Cullen published his discovery of the cold produced by the evaporation of fluids, (*Essays and Observations Physical and Literary*, vol. ii.) a phenomenon long known in Asia, but till then unobserved in Europe, and which has paved the way to so many other discoveries of the modern chemistry. It does not however appear, that Dr. Cullen applied his discovery in explaining the function of perspiration. The effect of evaporation from the surface in cooling the body, was however soon after observed by Dr. Franklin. In a letter, dated June, 1758, is the following observation: "During the hot Sunday at Philadelphia, in June 1750, when the thermometer was up at 100° in the shade, I sat in my chamber without exercise only reading and writing, with no other clothes on than a shirt and pair of long linen drawers, the windows all open, and a brisk wind blowing through the house; the sweat ran off

guin carry with them the highest authority, and it is deeply to be lamented that the death of Lavoisier, and the horrors of the revolution, cut short the experimental inquiry they had undertaken into all the principal functions of the human body, in health and in disease.

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the backs of my hands, and my shirt was often so wet as to induce me to call for dry ones to put on. My body however never grew so hot as the air, or the inanimate bodies immersed in the air.”* It does not indeed appear that Dr. Franklin actually measured his heat, but he makes an inference of his comparative coolness from remembering, that all the bodies about him, even his shirt out of his drawer, felt warm to the touch, and he concludes that he was kept cool, “by the continued sweating, and by the evaporation of that sweat.” Doubtless this reasoning was suggested to Dr. Franklin, and perhaps the whole circumstances recalled to his mind, by the paper of Dr. Cullen, then recently published. When, from the observations of Mess. Du Hamel and Tellet in France, and the experiments of Dr. Fordyce and Sir Charles Blagden in England, a discovery was made of the very extraordinary degrees of cold which the living body can support, philosophical minds naturally endeavoured to account for this singular a phenomenon. That the animal possesses a power of generating cold as well as heat, was the more general opinion: but the conclusiveness of the experiments of our English philosophers to establish this opinion was questioned by many, among others by my late ingenious friend Dr. Black, in the paper that forms the first number in

* See *Franklin's Letters and Papers*, p. 365.

the *Memoirs of the Society of Manchester*. In this paper the effect of the evaporation from the surface in reducing the heat during the experiments in question, is particularly insisted on. In discussing this subject with Dr. Bell, it suggested itself to me, that the principal office of the insensible perspiration might be to regulate the animal heat, and this opinion, which seems so reasonable, has been confirmed by reflection and observation.

But whatever may be the influence of perspiration in cooling the body, it is not perhaps the only process by which this effect is produced. From some experiments in the hot bath, it appears to me that the temperature of the body is with difficulty increased after the sweat begins to flow profusely, and as there can be no evaporation from the skin when the body is immersed in water, and a little from the lungs when the air inspired is already loaded with vapour, should my observation confirmed by future experience, it will perhaps shew, that in the production of sweat itself a degree of heat is absorbed, and thus explain in some degree the reduction of heat that follows profuse perspiration in other cases. It was indeed supposed by Albinus, Haller, and William Hunter, that the sweat, as well as the insensible perspiration, is an exudation of the watery part of the blood through

through the cuticle; but this opinion is contrary to all analogy, and depending on experiments made on the dead body, may be safely rejected, notwithstanding the illustrious names that countenance it. The opinion of Dr. Fordyce and Mr. Cruikshanks seems to be the true one. The matter of perspiration is separated from the blood by the capillary arteries, and thrown out on the surface by organic pores in the cuticle (however difficult to be discovered) connected with the extremities of these arteries; and in this process there is not a separation merely, but a new combination, as in similar instances. In this combination there may be an absorption of heat, and thus the coolness produced by sweating be in part accounted for. The absorption of heat may however take place in various other processes, besides the production and evaporation of perspirable matter; as doubtless its extrication takes place in various other processes besides respiration; and after all, the sudden changes of temperature that take place in certain circumstances, render it probable that the animal possesses powers over its heat not yet understood.*

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* Various ingenious arguments have been used by different friends, and correspondents, to shew the improbability of an absorption of heat in the formation of the matter of perspiration;

Leaving this point for future inquiries, the importance of perspiration to the animal œconomy we may safely affirm. By this process the stimulus on the extreme vessels, arising from morbid distension, is diminished or removed, and whatever other methods the system may possess of diminishing its heat, this discharge seems by far the most important, and in certain circumstances essential to that object. In situations where the organs of perspiration are spasmodically constricted, while by the increased momentum of the circulation, heat is preternaturally evolved (as in the commencement of fever) we can easily understand the disorder of the system that ensues.

It is observed, that persons who engage in excessive labour, speedily sink under it, unless they perspire freely, and support their perspiration by drinking some thin and moderately stimulating fluid. This is the case with the reapers in Pennsylvania, where the harvest occurs in the hottest

tion; this being a supposition which infers, that the matter of perspiration has a greater capacity for heat than the blood from which it is secreted, which is contrary to analogy. I have thrown out the suggestion, but would not be thought to rest any weight upon it. There are phenomena which occur in the sudden heating and cooling of the living body, which I am far from thinking we can account for by any known chemical principles.

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season of the year, and who, by means of profuse perspiration, are enabled to work in the sunshine, in which the thermometer rises very often above the heat of the blood.* This is the case with our coal-heavers, who probably lose a fifth or sixth part of their whole weight by this discharge daily, and who supply this waste chiefly by large draughts of porter. In instances of such extreme labour, sudden stoppage of perspiration from the cessation of exertion, and the accession of cold, is frequently fatal, as has already been explained.

Europeans who go to the West Indies are more healthy in proportion as they perspire freely, especially if they support the discharge by a moderate use of gently stimulating liquids, stopping short of intoxication, and guard against the effects of too excessive an evaporation when their vigour is impaired by fatigue. In the extreme heats of the torrid zone, this indeed is not easy. The highly excitable system of the youthful European is acted on powerfully by the climate. If the orifices of the skin do not pour out a proportional quantity of perspiration, disease must ensue from the direct stimulus of heat; and if the necessary quantity of perspiration takes place, the system is enfeebled by the evacuation, and the extreme vessels losing their

* See *Franklin's Letters and Papers*, p. 336.

tone, continue to transmit the perspirable matter after the heat is reduced as low as its natural standard, or perhaps lower—as occurs in the cold colligative sweats in the latter stages of fever. In this situation we can easily suppose, that even a slight degree of external cold may become dangerous. The damps and chilliness of night, joined to the increased evaporation produced by the night breeze, must often prove pernicious, especially after exercise and profuse sweating. They rob the surface of its necessary heat, produce a torpor and contraction of the extreme vessels, and the dangerous re-action of the centre, which has already been explained. The proper treatment of the European on his landing in the West Indies, may probably consist in his being placed for a time in some inland and elevated station, where the land being cleared, the air is pure, and less sultry than on the coast; on his avoiding as much as possible any violent exercise; on his guarding against all intoxicating liquids, and whatever increases the heat and diminishes the strength of the body—taking care however to supply the circulation with liquids proportional to the waste. By such means his system will gradually accustom itself to the climate, the organs of perspiration adapt themselves to the increased heat, and the dangers of sudden change be averted.*

* 1803. This reasoning would be much strengthened if we
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The perspirable matter of the European does not appear to be well fitted to the torrid zone.

Consisting

could rely on an observation of Dr. M'Kitterick Adair, quoted by Dr. Rush in the 5th volume of his Medical Observations and Enquiries, "that the heat of the European on his arrival in the West Indies, has been observed to be three or four degrees higher than that of the natives, or of those accustomed to the climate, to which however it gradually sinks in the course of time;" The experiments made by Dr. Chisholm, in Demerary, to ascertain this point, contradict this assertion, and lead us to suppose that it is one of those inconsiderate observations by which medical science is so often corrupted.

In 12 white persons newly arrived from Great Britain or Ireland, Dr. Chisholm found the mean heat, taken at the arm-pit, 96°.

In 12 white persons whose period of residence within the tropics was from 4 to 20 years, the mean heat was 96°. There is little or no difference when the trial was made on negroes; whether from Africa, or Creoles. These last however seemed to have the greatest heat, the medium of 12 of them being 98°, nor was there any difference in the heat arising from the period of life; infancy and age being of equal temperature.

See Dr. Chisholm's *Essay on the Malignant Pestilential Fever of the West Indies*. Vol. ii. p. 468.

If we could implicitly confide in the experiments mentioned by Dr. C. which give twice the proportion of oxygen gas to the atmosphere within the tropics, which it possesses in Europe; a different direction would be given to our enquiry respecting the effects of the West India climate on Europeans; but

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Consisting nearly of pure lymph, it is speedily dissipated by evaporation, and without a profuse discharge, the surface cannot be kept moist and cool. On the other hand, the profusion of the discharge debilitates the system, and renders even the cold of evaporation dangerous, especially when this evaporation is increased by an accidental breeze, or the effect of the cold promoted by rest after fatigue. The negro, fitted by nature to the climate, is less accessible to the stimulus of heat, and *his* unctuous sweat less easily dissipated, keeping his skin uniformly moist, sustains a more uniform perspiration, as well as evaporation, and guards the system against the waste and danger of profuse sweating under fatigue, as well as the other consequences already explained. The pungent and stimulating quality of his perspirable matter will promote these salutary effects. In this view of the subject, it may be worthy of investigation, whether the practice of the ancients of using unguents on the skin, still general among the eastern nations, and common among all savages, whose defective clothing obliges them to guard by this means against the vicissitudes of temperature, should not be revived by our countrymen in the West Indies. It has been imagined, that those unguents effect

where the result is so unexpected and extraordinary, repeated experiments are necessary to confirm it.

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tually obstructed the perspiration ; but this notion has been adopted without inquiry, and against probability. These unguents, like the perspirable matter of the negro, may be formed of a consistence, that, while it retards excessive sweating, does not obstruct moderate and necessary perspiration, and being themselves evaporable, they may keep up a coolness that shall diminish the necessity of the natural discharge. In the warm climates the tepid bath seems a necessary adjunct to inunction. It has been for some time used among the French in the West Indies, and is now (as we are informed) beginning to be adopted by the English ; and with proper restrictions, it may produce the happiest effects. After exercise under a burning sun, when the strength is wasted by fatigue and perspiration, immersion in the bath heated from 90° to 95° of Fah°. will be found most grateful and salutary. It restrains the profuse and colliquative sweating ; keeps up the heat of the surface and extremities ; and prevents the dangerous re-action of the centre, which a torpor of the extremities so generally produces. It allays the violent action of the heart and arteries, and soothes the system of sensation. On leaving the bath, friction, and inunction of the surface prevent the chill of evaporation, and while they ensure the safety of bathing, they prolong the pleasure of this classic gratification. The refinement of modern manners has introduced linen teg-
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ments next the body, and abolished inunctions of the surface. Hence the warm bath has become less safe, as opening the pores of the skin, and exposing the naked body to the chills of evaporation; and thus through the greater part of Europe it no longer forms a part of the regimen of health. In the east it still prevails. If it should be revived among our countrymen in the west, they must conjoin with it the inunctions of the surface, and the fine flannel teguments next the skin, which accompanied its use among the Greeks and Romans. In recommending a trial of unguents to the practitioners of the torrid zone, I may shelter myself under the authority of the celebrated Lord Verulam, unquestionably the first physician, as well as the first philosopher and statesman of the century in which he lived. “*Inunctio ex oleo, et hyeme confert ad sanitatem, per exclusionem frigoris, et æstate, ad detinendos spiritus, et prohibendam evolutionem eorum, et arcendam vim aëris, quæ tunc maxime est prædatoria. Ante omnia igitur usum olei vel olivæ vel amygdali dulcis, ad cutem ab extra unguendum, ad longævitatē conducere existimamus.*”*

* See *Fran. Bacon. Opera fol. Francofurti ad Manum*, 1665, p. 536, 537, 538, where the practice of various inunctions respecting unguents is considered at some length, and the cautions requisite in adopting this practice, lest the body should be overheated, &c. are very ingeniously detailed.

The operation of unguents on the surface of the body presents indeed a subject for important and original observation. The effects of the warm and tepid bath, though more investigated, are scarcely better understood, for perhaps there is no part of medicine on which so much has been written, and so little been decided. These subjects are connected together, and might form, with great advantage, part of an experimental inquiry into the laws of animal heat. The commonly received opinion, that the warm bath relaxes and enfeebles the system, must, I apprehend, be admitted with many restrictions. Immersed in water, or in air, heated to the degree that quickens the circulation, we are doubtless speedily enfeebled. But by a heat short of this effect, it may be disputed whether debility is ever produced. The degree to which the bath must be heated in order to quicken the circulation, approaches nearly to that of the blood. In my own case I have found the pulse become more frequent at 96° when the stomach was empty, but at 94° after dinner; and the practice, said to have been adopted by the ancients, of going into the bath to relieve themselves from the oppression of an overloaded stomach, would appear to me to have been attended with some hazard, and with great inconvenience. It does not indeed appear how the desired effect was obtained, except through the means of profuse sweating. In future experiments

experiments respecting the effects of the warm bath, it will be of importance to observe the heat at which the sensible perspiration begins to flow; which probably varies a good deal in different constitutions, and which as it varies, will materially affect the results. It will be important also to notice the state of the stomach as to fullness; the condition of the pulse; the previous degree of exercise; and the actual heat of the surface; all of which, according to my observation, will influence the experiments.

In all inflammatory diseases, it is of importance not to use the bath heated to the degree that materially quickens the circulation; where this is not attended to, the symptoms are heightened, unless indeed a speedy and profuse perspiration ensues. In the degrees in which it does not quicken the circulation, the warm bath is soothing and sedative, especially when the immersion is prolonged; and it is the temperature from 90° to 95° that is so singularly restorative after fatigue, though a still lower heat is safe and refreshing, as those who have used the baths of Buxton can testify.—The warm bath is frequently employed to excite a sensible perspiration, which may be prolonged after leaving it; and in this way it may be used with great advantage. Where this object is in view, it is advisable to immerse the patient in the water heated

heated to 94° or 95° , and very gradually and slowly to increase the heat to 97° , or perhaps 98° , watching its effects. When the sweat begins to appear on the forehead, if the pulse remains calm, and the patient feels at ease, an increase of heat to 99° , or even 100° , may be ventured on with safety, should profuse perspiration be required. But when the bath is heated *in the first instance to* 100° , the stimulus of heat generally produces a feverish circulation, which the subsequent defective perspiration cannot allay. The injurious effects are still greater where the bath is heated at first to 105° or upwards, and sweat, instead of flowing more freely in consequence of the increased temperature, is discharged with greater difficulty, the stimulus of the heat, and the suddenness of its application, inducing a contraction of the orifices of the extreme vessels of the skin. In this respect, as in most others, the analogy is perfect between the stomach and the surface; for it will uniformly be found, that sweating is excited more easily by draughts of *tepid* liquor, than an equal quantity of the same fluid swallowed as hot as it can be borne.

The sensible perspiration excited in the warm bath does not lower the temperature of the body while immersed, but being prolonged afterwards, it becomes powerfully refrigerant, and is a remedy of great efficacy. These observations however
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shew the importance of regulating the temperature of the warm bath by the thermometer, and demonstrate how little dependence can be placed on the observations of those writers who speak of its effects without noting its temperature. A variation of two or three degrees, often impossible to be ascertained by the sensations, will not merely vary the degree, but alter the nature of its effects.

The view given of the nature of fever, and of the functions of the perspiration, seems to afford an obvious explanation of the operation of those antimonial emetics, and sudorifics, by the early exhibition of which fevers are often stopped in their first stages. The direct action of these medicines on the stomach resolves the spasmodic stricture of the capillaries of that organ and of the surface, by which the morbid heat is confined; while the concussion given to the whole system dissolves the morbid catenation, and terminates the disease. These remedies are however uncertain in their beneficial effects, and always highly debilitating. They cannot enter into competition with the affusion of cold water on the surface, but may be employed when the presence of local inflammation precludes the use of that powerful remedy.

These general views cast some light also on that most curious subject, the operation of opium on the

the living principle. In former times it was disputed whether opium was in its nature cold or hot ; in our own days it has been disputed whether it is stimulant or sedative. Such abstractions afford exercise for the ingenuity, but in the way in which they have been conducted, they soar above utility and truth. Opium relieves pain, and produces sleep, and these effects being in general concomitant, may be treated of together. But though opium generally produces sleep, yet we are sometimes disappointed in this effect, and especially in those feverish disorders where sleep is of such high importance. About nine years ago, I attended a boy of ten years of age, the son of a friend, dangerously ill of fever. His nights had been sleepless, and the general irritation was very great. Opium had been administered in vain. I determined to give it in larger quantities, and to watch its effects. At ten at night he took forty drops of laudanum ; at twelve the same dose was repeated. As I stood by his bed two hours afterwards, he appeared in an imperfect and agitated sleep : his eyes were half open, his face was livid ; his lips and skin parched ; he was evidently much disturbed, and moaned in his breathing. At this time I perceived that he had tossed the cloaths off his feet, which lay naked, and that he grasped the cold bed-post with one of his hands. On examining his hands and feet they felt dry and burning. There was tepid vinegar
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in the room, and I moistened first his hands, and then his feet with it, without awakening him, repeating this practice from time to time: he became more quiet. Thus encouraged, I gradually moistened his legs and thighs in the same manner, and at length his breast and neck, and removing the rest of the bed-clothes, left him covered with a sheet only. In a little while the agitation subsided, and he sunk into a sound, and seemingly peaceful sleep, which continued several hours, the surface of his body being bedewed with a gentle perspiration.

This interesting case pointed out to me clearly, that the sedative effects of opium are often counteracted by the stimulus of heat on the surface and extremities, and that the actual state of the heat of the patient is a circumstance requiring particular attention in the administration of this powerful remedy. The following observations are the result of the inquiry which these reflections suggested.

Opium given in health generally produces sleep, *if the mind be vacant, the stomach empty, and external impressions be excluded.* In this case, its first effect is to increase sensibility, to give a gaiety and liveliness to the imagination, and to diffuse a genial glow over the surface and extremities. The actual heat of the trunk of the body is little if at all increased, but the surface and extremities are

brought up to the general temperature, (97° or 98) and a gentle, and as it has been called, a breathing perspiration is diffused over the skin. In this state we sink into those happy slumbers which are ill exchanged for the realities of life. On the approach of sleep, the pulse is generally increased in frequency by four or five pulsations in the minute, and the respiration is slightly irregular, as in the gentler degrees of pleasurable emotion; but as the sleep becomes fixed and profound, the pulse subsides to its lowest and most tranquil state, and the respiration becomes regular, deep, and rather slower than natural. The various circumstances that prevent this favourable operation of opium, even in the state of health, it is not our purpose to consider at present.

When opium is given in fever, if the heat be two or three degrees or upwards above the natural standard, and the skin dry, it seems very generally to increase the heat and restlessness. There are exceptions. If the heat, though preternaturally great, is subsiding, and the skin beginning to moisten, though not yet moist, opium very often accelerates the perspiration, and by this means diminishes the heat. In such cases its salutary effects, tranquillity and sleep, generally follow. Thus it will happen, that an anodyne draught given early in the evening, shall occasion increased

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heat and agitation, which if deferred till two or three in the morning, would have produced sensible perspiration and repose. In the evening the exacerbation of fever is on the increase, or at its height, which towards morning is subsiding, the difference in the actual heat of the surface being often not less than two degrees or upwards. A dose of opium given in the hot stage of intermittent fever, will often accelerate the sweating stage and shorten the paroxysm, which under similar circumstances in continued fever, has a contrary effect; because in the former case the disposition to perspire is more easily excited. But even in intermittent fever, opium when given in the hot stage, will be much promoted in its diaphoretic and salutary effects, by moderate draughts of tepid, or if the heat be great, of cold liquids. In continued fever, where the heat is great, and the skin dry, it is proper to lower the temperature of the surface, and if possible to excite sensible perspiration before opium is administered, if we wish to ensure its diaphoretic and soporific effects. But even after opium has been exhibited, when inordinate heat prevents its sedative operation, it will be found safe and salutary to use the tepid or cold affusion; and when the heat is by this means reduced, repose and sleep will follow. Tepid or cold drink will produce, though in a weaker degree, similar benefit. These methods of promoting the diaphoretic effects of opium,

opium, seem more certain and advantageous in fever, than the practice of combining it with ipecacuanha, or the preparations of antimony; but where opium is to be used in inflammatory diseases, or in dysentery, doubtless this last method is to be preferred. These remarks must be considered as applying to opium in its ordinary doses, that is, from half a grain, to two or three grains of the extract, or from ten to sixty drops of the tincture. Perhaps it has been too much the practice of late to give this medicine in large doses, and to overlook its effects in smaller quantities. Experience has convinced me, that considerable effects are produced on the system by a very few drops of the tincture properly administered, and that it is always unwise to employ it in doses larger than necessary to produce the desired effect.

The effects of alcohol (under which term I comprehend the spirituous and vinous liquors) on the system, have a striking resemblance to those of opium. Alcohol is more heating, less diaphoretic, and less soporific. When however *the mind is vacant, the stomach empty, and external impressions excluded*, alcohol like opium has a tendency to induce sleep. As sleep however approaches, the heat of the body rises, especially on the surface and extremities, and stimulating the heart and arteries to increased action, produces a state of agitation with

which sleep is often incompatible. Or if the dose has been such as to benumb the senses, with the first return of sensibility, the drunkard is roused by intolerable heat from his apoplectic slumbers. The degree of heat in the fever of intoxication is doubtless various in various constitutions. I have observed it to rise to 105° , nearly the highest degree of heat in the typhus of this island; and Dr. Alexander in his own case found it as high as 107° .—Under these degrees of heat, the thirst, agitation, and consequent debility, are very considerable, the skin being in general obstinately constricted. The proper treatment in this, as perhaps in all other cases of morbid heat and dry skin, when local inflammation is absent, is to pour large draughts of cold water into the stomach, or to affuse it on the surface. By this practice the heat is reduced, the orifices of the skin opened, sensible perspiration is produced, and one half of the mischief of intoxication is prevented. Similar effects may be produced, though in a weaker degree, by tepid drinks or the tepid affusion.*

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* 1803. The following narrative, which was communicated to me as illustrative of these observations, I give in the words, and on the respectable authority of Dr. Robertson, late Surgeon-general of the naval hospital in Barbadoes.

“ A Gentleman of this Island, whose name was Weeks, a
“ great

The administration of alcohol in fever requires the same precautions as that of opium. It should not be given when the heat is great, and the skin dry

great votary of Bacchus, was in the practice, from fifteen to twenty years, of plunging into cold water when he rose from his bottle, and of actually going to sleep in a trough full of water, with his head supported on a kind of wooden pillow made for the purpose, above the surface. When he dined abroad, and had not the convenience of his own trough, he used to strip off his coat, waistcoat, and shirt, and sit exposed in the open air, and in that situation go to sleep, whether it rained or not. And sometimes he went and bathed in the nearest adjoining pond, to which he generally required assistance to be conveyed. The effect of this practice was, that instead of experiencing debility, lassitude, headache, and nausea, he found himself on awaking, cheerful and refreshed, and free from all the effects of intoxication. In the year 1789, dining one day abroad, he got alternately drunk and sober three several times before midnight, each time recovering his sobriety by immersing himself, and sleeping in cold water; and on awakening returning to the company. The last time, after supper, he was so immoderately intoxicated, that he insisted on his companions undressing him and carrying him themselves to the pond. They carried him accordingly in a chair, and set him up to the chin in water, where he continued upwards of an hour, a person supporting him. I had this last circumstance from a Gentleman one of the party, whose veracity may be entirely depended on.

“ At home however he used, as I have already mentioned, a trough made for the purpose, with a bench in it as a pillow,

dry and burning: but it may in general be given with safety and advantage, when the heat is nearly of the natural standard; and even when it is somewhat

“ low, having been nearly drowned when sleeping in his pond,
 “ from the negroe who was appointed to watch him having
 “ himself fallen asleep. In this watery bed he would sleep,
 “ one, two, three, or even more hours, experiencing always
 “ the greatest refreshment. His wife and family, when they
 “ wished him to change his quarters, used to draw out the plug,
 “ and let the water run off, when he awoke, and humorously
 “ complained of the loss of his bed-clothes. At length this ex-
 “ pedient began to lose its effect in rousing him, and one time,
 “ he continued to sleep in his empty trough. In consequence of
 “ this he was seized with extreme rigours and chills, followed
 “ by a severe attack of rheumatism, which affected him a long
 “ time, and made him desist from the practice in future.
 “ But to the end of his life he was in the habit of sitting,
 “ when intoxicated, with his clothes open, and sometimes
 “ quite naked, exposed to the wind and rain. This extraor-
 “ dinary character died of apoplexy about three weeks ago,
 “ aged sixty-threc.”

*Extract of a letter from Dr. Robertson, dated Barbadoes,
 4th March, 1801.*

Dr. Robertson had mentioned to me generally the circumstances above related, in conversation, about a year before, and at my request promised to investigate the particulars on his return to Barbadoes. The above is the result. Mr. Weeks was alive many months after Dr. R.'s return, and this narrative of his practice may be entirely depended on. It would afford room for many observations. I shall offer one or two.

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what above it, if the orifices of the skin are open, and the surface is moist. Opium, as was before remarked, is more sudorific than alcohol, and given in

Should any one be desirous of following Mr. W's. practice, he must recollect, that the cold water of Barbadoes is from 76° to 80° , 'little inferior in temperature to the Buxon Bath. t. 4 The superior coolness derived from immersion in water of this temperature, to that in air of the same heat, arises from the greater specific gravity of the water, and its much greater power of conducting heat. An attention to this circumstance might be of the utmost advantage, not merely in diseases of encreased temperature, but when the living system, though languishing under excessive heat in the sultry regions of the earth, may be still considered as free from disease. We learn from Dr. Chisholm, who served with the British army in South Carolina and Georgia, in the American war, that in the inland part of these countries, where the atmosphere stagnates during the latter end of summer, and the heat rises to 90° and upwards the inhabitants often take refuge from the intolerable oppression of the air, by immersing themselves in water. This practice ought to be known to our countrymen in the East Indies. I listened with very painful sensations to the account of the sufferings and death of an officer of great merit and high connections, who, when recovering from an illness seems to have sunk under the excessive heat of a stagnating atmosphere in that country. An attempt was made to relieve him, by pouring water on a bundle of wet hay in his chamber; but in a stagnating state of the air, the degree of coolness thus produced would be unequal to the object. Had he been immersed in the water, or had the water been poured over his body, and the air, by artificial means, been made

in that stage of the fever of intoxication in which the skin begins to relax, it often accelerates sensible perspiration, and sometimes occasions it, when it might not otherwise have taken place. In this way I explain a phenomenon which has occurred to others as well as to me, that a moderate dose of opium sometimes gives extraordinary relief to the drunkard, under the sufferings through which he must pass to sanity and strength.

These observations are not offered to the reader as including a complete view of the subject of which they treat. My principal object has been

made to blow over him when wet, the relief would in all probability have been effectual. Of these two methods, the first, where the person is feeble, is the safest, and least exhausting. As a cooling process, within the higher degrees of heat to which the atmosphere rises, the last is the most effectual; but the cooling power of evaporation may be carried too far, and requires skilful management. Dr. Chisholm assures me, that, in the West Indies, he has known the heat of the atmosphere suddenly reduced by evaporation, forty degrees.

A consideration of this circumstance will explain why Mr. Weeks suffered when the water was drawn off from his trough, and he lay wet and naked, exposed to the wind. Supposing the water and the air to have been of the same temperature, the greater density of the former was more than compensated by the effects of evaporation; something may also be imputed to the change of medium itself, as explained chap. xv.

to establish the use of a new and powerful remedy in fever, and the general remarks and reasonings which are added, are chiefly with a view to explain its operation and enforce its safety. As however these additional observations are on subjects of great importance, it is my intention at some future period to resume them. In the mean time, it may be objected, that in this rapid sketch, assertions are sometimes given instead of experiments and proofs. In such cases however, it will be found, that the points at issue may be readily brought to the test of experiment; and as general and obscure expressions are every where avoided, it will be easy to resort to this trial; a circumstance which will procure from candid minds a patient hearing, as well as forgiveness for those errors into which I may have fallen,

I have only to add, that the application of cold under any form, where fever is combined with local inflammation, is a subject of much difficulty, and my observations upon it must wait for the acidations of future experience*. For the same reason

1798. * Finding that some of my correspondents are disposed to extend the affusion of cold water to symptomatic and inflammatory fevers, and that they are even inclined to think, that in such affections it must act with peculiar advantage, I submit to this ~~third~~ edition, a few observations on this point, but *referred* rather

reason I have avoided a particular examination of the action of cold on the body in health, whether
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rather with a view of inculcating caution than the hope of directing practice.

That the action of cold on the living body is under certain circumstances stimulating, is maintained in *note*, p. 73, to which the reader is referred; and this stimulation depends on the sensation by which the application of cold is attended. In fever with local inflammation, there is a morbid degree of heat, for which the application of cold would seem the natural practice. But in such diseases the sensibility to cold (as well as to other powers that stimulate) is also much increased, and hence the difficulty of resorting to it as a remedy. Cold acting powerfully on the sensations, will in the first instance stimulate the system; and it appears to me probable, that in proportion to this stimulation, is the difficulty of reducing the actual temperature. Hence it may happen, that the injudicious application of cold in inflammatory diseases, shall increase the action of the heart and arteries by the stimulus of sensation, without subtracting *permanently* the stimulus of inordinate heat. This observation extends to measles, catarrh, &c. in which a sense of cold is so easily excited.

To apply cold with success in inflammatory diseases, it seem to me probable, that the sensation which attends it should be as much as possible moderated, and that the application should be so regulated as to insure the effectual reduction of the temperature of the patient. If this be the case, the sudden and temporary affusion of cold water, so advantageous in typhus, will not be salutary, or indeed safe, in inflammatory fever, in which it appears to me that the patient, if
cold

as a strengthening or a debilitating power, an inquiry that ought to be first entered into, in a regular

cold is employed, should sink himself slowly in the cold bath, continuing the immersion for such a duration as the state of his heat, and of the pulsation of his heart and arteries will admit. By the experiments in chap. xv. it appears, that under continued immersion in cold water, the sensation of cold speedily abates, and that the frequency of the pulse is in a most striking and uniform manner reduced. And though the heat in the trunk of the body is wonderfully sustained, yet in the extremities, and over the surface, it is speedily and permanently lessened. It is true these effects may be modified by disease, in such a way as to contradict experiments made on a person in health; and though in pure synocha, the cold bath would probably be advantageous, yet serious objections may present themselves against employing it in pleuritis or peripneumonia; objections which I do not think insurmountable, but which experience neither enables me to invalidate ~~nor~~ to strengthen. *non* In phrenitis, gastritis, enteritis, &c. especially in their desperate forms, I think immersion in the cold bath ought to be hazarded.

It may serve to support this reasoning, that the local application of cold to parts inflamed, is regulated by the principles I have laid down. It must not be sudden and temporary, but great in degree, and permanent in duration. Hence the success with which ice, and snow, and the clay-cap, are applied to different parts, for the purpose of preventing or reducing inflammation. In all such cases the sensation of cold speedily subsides, and even though ice be lying on the part affected.

It is well known that the general action of cold may be extended over the system by its application to a part. The use of

gular investigation of the effects of temperature, and its changes, upon life.

of cold in hemorrhagies is often regulated by this maxim. I have found that hemorrhagy from the lungs may be stopped by immersing the feet in cold water, and perhaps this may be done still more certainly by a permanent application of cold to the penis and scrotum, which part with their heat more easily than any other portion of the surface of the body. I have found that a still more powerful effect was produced in hemoptoe, by immersing the body up to the pubes in cold water, a practice that I can speak of from experience as often safe and efficacious in this disease. But in all these instances, a permanent, as well as powerful application of cold was requisite, and from the analogy between hemorrhagies and inflammations, this may support our reasonings respecting the proper method of employing cold as a remedy in these last diseases.

CHAP. XIX.

*Is there an inhalation by the Skin? Case of
P. M. Esq. who died of Inanition. General
Reflections.*

IN speaking of the origin of animal heat, and of the nature and office of perspiration (*p.* 266) it was questioned, whether the inhalation of vapour or water through the skin into the circulating fluids, be established by a sufficient induction of facts, and if it occur at all, it was supposed that the quantity inhaled had been much exaggerated. Among the ancients however, the doctrine in question was universally received, and it has been admitted by the most celebrated physiologists, not merely of the past, but of the present times. The facts and arguments in support of it are detailed by Haller in the 5th volume of the *Elementa Physiologiæ*; but on examination, they will be found to afford
no

no direct proof of its truth*. Of the great variety of authors quoted on this occasion, no one indeed affirms the actual increase of the weight of the body in the warm bath, Dessault excepted, and this is a point that Haller himself seems never to have subjected to experiment.

The inhalations through the surface in vegetables, in insects, and in certain of the amphibia, are the analogies that give the most direct support to this doctrine, applied to the human species. It is however controverted by many facts, and if it should ultimately be established, it must be reared on a new foundation. When I was at Buxton in 1788, I made an experiment on the effects of bathing on the weight of my body, and after half an hour's immersion, I found it rather diminished than increased. This experiment had I believe

* The arguments drawn from the appearances that occur in the dead body, may be wholly set aside; as may the instances of certain narcotic vegetables, applied to the skin, (e. g. tobacco and white hellebore) occasioning sickness at the stomach, which are cases of sympathy, or of absorption *after the cuticle had been eroded*. The arguments drawn from the increase of weight in moist air, and from the vast discharge of urine in diabetic cases, are also wholly fallacious. In diabetes we know there is no absorption by the skin, and the phenomenon in question, which has been much exaggerated, must be accounted for by the action, not of the skin, but of the lungs.

been made before by Dr. Pearson, with the same result; it has since been repeated frequently with great care*, and it is ascertained as a fact, that no increase in the weight of the body is produced by immersion in water, of the heat of 82°. In the year 1790, I had a patient in diabetes, whose cuticle, as is usual in that disease, was in a morbid state; and being desirous of trying how far the inordinate action of the kidneys might be affected by a gentle stimulus applied to the skin, I immersed him in a bath of the temperature of 96°, weighing him before and after immersion. There was no sensible variation in the weight. This experiment has since been made by Dr. Gerard, in another case of diabetes, an account of which is given in the publication of Dr. Rollo†; and as it was repeated a great number of times with the utmost care and accuracy, it may be considered as established, that immersion in the warm bath in diabetes produces no increase of weight. I have made five different experiments of the same kind on myself, varying the heat of the bath from 87° to 95°, but never in any instance found my weight augmented. It may be said however, (and it has been said) that though in

* In one case a sum of money depended on the issue, and a friend of mine submitted to be the subject of the experiment.

† Rollo on *Diabetes Mellitus*, v. ii. p. 72.

diabetes,

diabetes, where the epidermis is diseased no liquid is inhaled ; and though in health, where the vessels are full, no absorption may take place, yet that where the body is wasting from a want of the proper food through the stomach, the compensating powers of the living system may be exerted to supply the defect, and to excite an inhalation by the absorbents under the cuticle, or through those pores on the surface, by which exhalation is usually performed.

The following case will throw some light on this point. I give it however not merely with this view, but as containing other particulars that appear to me curious and interesting.

In August 1795, a gentleman of Yorkshire, aged 66, applied for my assistance in a dysphagia, (obstructed deglutition) with which he had been for ten or twelve months afflicted. At first the complaint was slight, and occurred only when he attempted to swallow dry and hard substances ; it afterwards extended to solids of every kind ; and at the time I first saw him, though he was still able to pass down liquids, the quantity he could swallow was not sufficient for his nutrition, and he was considerably reduced. On the introduction of a bougee into the gullet, it passed about two inches easily, but then met with an obstruction, which by
a moderate

a moderate pressure was overcome. It then passed easily seven or eight inches more, but experienced a firm resistance in the lower part of the tube, towards its termination in the cardia, which the skill and patience of Mr. Park could not surmount, and which finally baffled every resource of his art. Though we had not an opportunity of examining the œsophagus after death, yet the nature of this obstruction was evident. It doubtless proceeded from a schirrous tumour, gradually increasing, which at first diminished the passage, and in the end obstructed it wholly; and it existed in that part of the tube, which, from its suddenly narrowing, is, as Dr. Hunter has remarked, most exposed to the causes, and most frequently the seat of this disease. Mr. M. never had any considerable pain from the pressure of the bougee on the tumour, and though he expectorated (if the expression may be used) much mucus, there never was any purulence discharged, or any reason to believe that the tumour had ulcerated.

The last time Mr. M. swallowed in any quantity, was on the 17th of October. On the evening of that day a sudden increase of the obstruction came on, and from this time forth he was able to swallow only a table spoonful of liquid at a time, and with long intervals. It was with difficulty that he got down seven or eight spoonfuls of strong

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soup

soup in the day, and this quantity gradually diminished. On the first of November, the passage seemed wholly obstructed. As he wasted rapidly, we were obliged, from the 18th of October, to employ the subsidiary modes of nutrition, and he went into the tepid milk-and-water bath, and had a nutritive clyster administered once a day.

As Mr. M. himself was far from despairing of his recovery to the last, the affectionate friends around him did not wish him to be informed of his real situation. He continued therefore to swallow two or three table spoonfuls of milk daily, till about the 15th of November, on the supposition that some might pass; but though it rested in the oesophagus some time, it was constantly ejected at last; and being fatigued by this expectoration, which disturbed his rest, and by which he perceived that the whole of the milk was thrown up, for the last twenty days of his life he made no attempt to swallow, but contented himself with washing his mouth and fauces with wine and water.

Mr. M.'s friends, though they could not but foresee the issue of his case after all attempts to remove the obstruction had failed, yet desired that his life might be prolonged to the uttermost. The following plan was therefore adopted with this view, on the last days of October, and it was per-

severed in with the variations afterwards to be mentioned, till the commencement of the delirium that preceded his death.

Every morning at eight o'clock, he had a clyster, consisting of eight ounces of strong broth, made chiefly of membranous parts of beef (being the most nutritious, according to Dr. Fordyce) into which were rubbed two yolks of an egg, and to which were added 40 drops of laudanum. This was repeated at three in the afternoon, and again at nine in the evening; but in the evening, instead of forty, one hundred and twenty drops of laudanum were added, to compensate the want of an anodyne draught, to which he had been accustomed at the hour of rest. Previously to this however, he was placed up to the neck in a tepid bath, of which one fourth was milk, and the rest water; the whole quantity amounting to twenty-four gallons. The temperature was fixed at 96° to accommodate his sensations, and the time of immersion was gradually prolonged from forty-five minutes to an hour.

The size and repetition of the clysters were determined by our experience of what the rectum would retain; the laudanum being added to increase its retention, as well as to allay irritation, and to stimulate the living energy. In a few days

the retention of the rectum improved, and enabled us to enlarge the clysters to 10 oz. of broth, and three yolks of eggs each; to which, from the 12th of November, 8 oz. of white wine were added; the laudanum being increased to 60 drops for each of the clysters in the day, and to 150, 180, 200, and 250 drops for the evening clyster. Thus the whole of his nutriment for twenty-four hours consisted of 30 oz. of broth, 24 oz. of wine, 9 yolks of eggs, and from 250 to 380 drops of laudanum, by clyster; with what liquid might be supposed to be taken up by the absorbents of the surface in the bath. Mr. M. was very tall, and naturally corpulent. In health he had weighed 240lbs. But imperfect nutrition had gradually reduced him, and at the time of commencing this process, he only weighed 179lbs. On the 20th of November he was reduced to 154lbs.; on the 25th to 149lbs. This was the last time we had an opportunity of weighing him. He seems to have wasted about 5lbs. in four days, and on the second of December, when his delirium commenced, he probably weighed 138 or 139lbs. having lost upwards of 100lbs. of his original weight. He lived till the 6th of December, but from the 2d, the rectum no longer retaining the clysters, they were of course omitted, as well as the bath.

About the 25th and 26th of October, there

was reason to suppose that Mr. M. would have had a shorter date. He had swallowed a very small portion of nutriment for the preceding eight days, and the plan of nutrition just mentioned, though adopted in part, was not fully established. His tongue and lips were at this time become dry; a blister applied to the sternum, discharged a thick coagulable lymph only; and his urine, reduced to a few ounces in the twenty-four hours, was become extremely high coloured, and almost intolerably pungent. In a few days however this state of the tongue went off: the skin became perfectly soft, and was often covered with a gentle moisture; the urine flowed without pain, and of the natural appearance, the quantity being from 24 oz. to 36 oz. in the twenty-four hours—more than the whole of his loss of weight in the same space of time.

Mr. M. had in general a stool after every fifth or sixth clyster, that is, three or four times a week. The substance of these stools was solid, and nearly of the natural fœcor, and the mass perfectly homogenous. The colour was lighter than natural, as is the case in obstructions of the bile duct; their whole appearance resembling what we find in cases of dissection, in the termination of the Ileum or beginning of the Colon, with the exception of being much less liquid. The concoction of the fœces, if I may so speak, was much more perfect towards the

the end, than at the beginning of this period ; at first they looked curdy, but at last they approached to the natural appearance.

The heat, examined by the thermometer, was natural, and nearly uniform from first to last. After the clysters there was generally some flushing of the cheek, and drowsiness, with some increase in the warmth of the extremities ; but under the axilla there was seldom any variation, and when it rose there a single degree, it was speedily lowered by a gentle diaphoresis.

The pulse, during the period of the scarcity and pungency of the urine, was disturbed, and more frequent. It afterwards subsided to the natural state, and during the whole of the month of November, was not to be distinguished from that of a person in health. On the morning of the first of December it became small and frequent ; and still more frequent, though stronger, after the delirium commenced ; the state of the pulse, as well as almost all the other symptoms, from that time forth, very nearly resembling the symptoms of the last stage of fever, when it terminates fatally. During this delirium, a perpetual and indistinct muttering occurred, with great restlessness and agitation ; the surface and extremities were sometimes of a burning heat, sometimes clammy and cold. The eyes
lost

lost their common direction, the axis of each being turned towards the nose. In this state however the sensibility of the retina was not impaired, but rather encreased, for he screamed out on the light of the window being admitted, to which before he had been accustomed. At this time also the sense of touch seemed more than usually acute, for he appeared disturbed with every accidental breath of air. The delirium, and the derangement of vision commenced nearly together, but we observed the derangement of vision first. On the first of December he complained that he sometimes saw double; but it was not till the succeeding day that any considerable incoherence of mind was observed. The pulse became feeble and irregular on the fourth; the respiration, which had been singularly undisturbed, became laborious; the extremities grew cold; and in ninety-six hours after all means of nutrition, as well as all medicine had been abandoned, he ceased to breathe.

Mr. M. complained very little of hunger. Occasionally he expressed a wish that he could swallow, but not often, nor anxiously; and when questioned on the subject of his appetite, always declared that he had no hunger to occasion any uneasiness. The clysters evidently relieved the sense of hunger, and the opium they contained, seemed to have a principal share in producing this relief.

relief. It occasioned quiet and rest after each clyster, and allayed every kind of desire or appetite.

Neither was Mr. M. much disturbed with thirst. This sensation was indeed troublesome during the first days of his abstinence; but it abated, and, as he declared, was always removed by the tepid bath, in which he had the most grateful sensations.

Mr. M.'s spirits were uncommonly even, and his intellect perfectly sound. He occupied himself a good deal in his private concerns, and as usual interested himself in public affairs. As we discouraged all unnecessary exertion, he spent a great part of his time in bed; but till the last few days of his life, he dressed and undressed himself daily, and walked, not only about his room, but through the house. His nights were quiet; his sleep sound, and apparently refreshing. Towards the end of this period, he said he had very lively dreams, but they were all of a pleasant nature. The last conversation I had with him was on the morning of the second of December. He told me that he had had a very gay evening with two Yorkshire Baronets, whom he named; they had pushed the bottle about freely; many jokes had passed, at the recollection of which he laughed heartily,

heartily, a thing uncommon with him. When I observed that this was a very lively dream, he seemed to endeavour to recollect himself, and for a moment appeared sensible that the scene had passed in sleep. But recurring to the circumstances, he spoke of them again as realities, and I made no further attempt to undeceive him. He said he had been walking out that morning, and was pleased to see the spring so far advanced—he had never felt the air so refreshing. At this time, though his pulse was become rapid, his respiration was natural, and his speech distinct, and without effort. He was extremely weak, and scarcely able to turn himself in bed, to which he had for several days been entirely confined. No man had ever perhaps approached death by steps more easy; and it was earnestly to be wished that his remaining moments might be few. This wish was not gratified. In about an hour afterwards his delirium became complete, during which his strength evidently increased, and the struggle commenced which has already been described, and which lasted ninety-six hours. He died in his sixty-seventh year.

I have been the more particular in noting this case, because I have not found the methods of supporting life under impaired or obstructed deglutition, sufficiently detailed, nor the death from inanition

dition described with sufficient accuracy. A knowledge of the first may on many occasions be important; and an acquaintance with the last may possibly throw light on some difficult questions that respect not merely the vital, but the intellectual phenomena.

I have not found any case deserving of credit, in which life was so long preserved without the aid of the stomach, two excepted, one recorded by Ramazzini*, and the other mentioned by Mr. Cruikshanks†. The case described by Ramazzini was that of a nun; and the obstruction was not mechanical, but spasmodic, or as he himself supposes, arising from a paralysis of the œsophagus. She is said to have lived sixty-six days without swallowing, the whole of her nutriment consisting of a single clyster of broth, and two yolks of eggs administered daily. The dysphagia, which came on suddenly, seems also to have gone suddenly off, and she finally recovered. The case is not given with such minuteness by Ramazzini as to inform us whether any attempts at swallowing were made during the dysphagia. As the obstruction was not

* *Ramazzini, Opera Omnia Geneva, 1717. p. 178.* Many hundreds of fabulous relations of long continued abstinence may be found, some of which are noticed by Haller.

† *The Anatomy of the Absorbing Vessels, London, 1786. p. 10.*
mechanical

mechanical, it is probable that such attempts were made daily; and if this be supposed, a suspicion may be entertained, from the nature of the affection, that more or less nutriment might be swallowed. It is observed by Ramazzini, that the sufferings of his patient ~~was~~ so great on the 30th *were* day, that she was anxious for death, and refused to admit the clyster, which however was administered during syncope, and with immediate and happy effects. In this respect her situation was perfectly contrasted by that of Mr. M. who enjoyed ease and serenity of mind till the last struggle, as has already been mentioned.

The case of Mr. Cruikshanks is not given at length, but alluded to in the following words: "That the surface of the skin absorbs other fluids which come in contact with it, I have not the least doubt. A patient of mine, with a stricture of the œsophagus, received nothing, either solid or liquid, into the stomach for *two months*; he was exceedingly thirsty, and complained of making no water. I ordered him the warm bath for an hour evening and morning *for a month*; his thirst vanished, and he made water in the same manner as when he used to drink by the mouth, and the fluid descended readily into the stomach." It is to be regretted that a case so curious is not given in detail. It does not appear

appear what other means of sustenance Mr. Cruikshanks employed besides the tepid bath, especially during the month of abstinence, in which the bath itself does not seem to have been used. On some future occasion it is to be wished that he may supply these deficiencies, especially as the information I am about to give will shew, that the conclusion he draws from this case of the absorption of the skin, is fallacious*.

Notwithstanding what I had observed formerly, I fully expected in the case of Mr. M. an increase of weight from the immersion, because he always expressed great comfort from the bath, with abatement of thirst, (which however was never great) and because subsequent to the daily use of it, the urine had flowed more plentifully and become less pungent. I expected it also from the authority of general opinion, and was curious to know the extent of the absorption. I weighed him three different times. The first time he was weighed with his clothes on, before he went into the bath, and naked when he returned from it. Our process was therefore complicated. He appeared to have

* 1803.—I hoped to have had a communication from Mr. Cruikshanks on this case, as I applied to him, through a friend, on the subject, but I was unsuccessful. He has since paid the debt of nature. I do not consider his case of any authority,

gained 30 oz. in weight, which gave him much comfort, and this circumstance prevented me from undeceiving him when I afterwards discovered the error*. The second time he stepped perfectly naked upon Merlin's balance immediately before immersion, and again immediately after it, his body being previously dried. The weights were never moved. The result surprised me: I could not distinguish the slightest variation in the weight of his body, though my beam would have detected a single drachm. I repeated this experiment four days afterwards, and with a similar result.—On both of these occasions the immersion continued an hour, and a constant friction had been kept up nearly the whole of this time on the inner surface of the thighs, with the view of stimulating the absorbents.

It is worthy of observation, that there was neither increase nor decrease of weight in the bath; yet Mr. M. was at this time wasting twenty ounces in twenty-four hours, and consequently, if there was no absorption in the bath, it might be supposed that he would have lost $\frac{5-6}{10}$ ths of an ounce during the hour of immersion. It is the more remarkable that there was no decrease, because in the bath the skin was softened, and the forehead became covered

* I discovered that we had been incorrect before the second experiment.

with

with a gentle moisture, which probably arose from the condensation of the vapour from the bath. At the time, however, I considered this as the matter of perspiration, because, after leaving the bath, and going into a bed previously warmed, he usually fell into a gentle and general diaphoresis, the soother of every irritation, and the harbinger of refreshing sleep. So far the case is tolerably complete. I lament that I cannot give the appearances on dissection, as we were not permitted to examine the body after death.

The reflections suggested by this case are numerous: I shall present one or two that seem important.

The natural state of the pulse during the first month of Mr. M.'s abstinence, when his strength was daily decaying, demonstrates, that the motion of the heart and arteries gives no certain indication of the strength of the system. Pure debility within certain limits, does not seem to produce a frequent pulse, nor in all cases a feeble one; but when carried to a certain degree, it generally excites that commotion of the system, in which — first the contractions of the heart, and afterwards of the lungs become more rapid, which in popular language may be denominated the last struggle of nature

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and which may be considered as indicating the approach of death*. To endeavour to reconcile these phenomena to any of the prevailing theories of vital motion, would be a fruitless attempt. Death has not an uniform aspect, but its *phases* do not appear to me numerous. The concluding scene in the life of Mr. M. may illustrate the form of its approach, as well as the nature of the symptoms in the last stage of fever, and of various other diseases, in which similar symptoms occur.

* In a recent case (April 1798) where deglutition was obstructed from a different cause, the same methods of supporting life, as in the case of Mr. M. were resorted to. But the tepid bath was soon abandoned, the patient conceiving that it irritated a cough to which he was subject. In this instance, as in that of Mr. M. the approach of death was gentle and easy. The patient, who was aware of his real situation, met his fate with perfect equanimity, and possessed his understanding to the last. Though the pulse became more frequent two or three days preceding his death, yet there was little or nothing of that struggle described in the case of Mr. M. This gentleman had long been wasting under an incurable disease, and had taken little nutrition for some time previous to the commencement of his abstinence. He lived twenty-three days after he desisted from all attempts at deglutition. He never complained of hunger, and the thirst which he felt occurred at intervals only, and was not very severe. The case was unfavourable to more particular observations; the patient having been long subject to a disease of the system, by which the epiglottis and the surrounding parts were finally destroyed, his symptoms were of a mixed nature.

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The natural state of the heat during the inaction of the stomach, might prove, if it were necessary, how little the vital heat is connected with the functions of digestion, and renders it probable, that the general increase of heat which follows the application of many stimulating bodies to this organ, is not the direct effect of the action of the stomach itself, but of its influence on the heart, or diaphragm, or both. It is asserted by Haller, that, in cases similar to that of Mr. M. the heat has been found diminished. The observations made by the thermometer in this case throw a doubt on the assertion, while the great facility with which Mr. M. parted with his heat, concurs with a variety of corresponding facts to support the position, that in those situations, where the generation of heat is natural, the power of retaining it in the system is in proportion to the force of the living principle.

But what shall we conclude respecting the absorption of water by the surface of the body in the warm bath?—Admitting that it did not take place in the case of Mr. M. it may be supposed this did not arise from the impermeability of the cuticle, but from the heat of the bath being too high. It may be added, that the analogy of the vegetable kingdom supports this supposition, since vegetables are found to exhale in warm and dry air during the day,

day, but to absorb largely under the moisture and coolness of night. Considerations of this kind might probably influence some learned physicians in London, who were consulted in this case, to propose that the heat of the bath should be as low as 80° .—It was however raised to 96° for two reasons: the first, that I had found no increase of weight in the Buxton bath at 82° the second, that as Mr. M. parted with his heat easily, he could not support immersion with comfort in a lower temperature.

It is however more likely to be supposed, that though there was no increase of weight in the case of Mr. M., there might be an absorption in the bath, which was countervailed by the increased exhalation.—The experiments I have detailed do not enable us to decide this point absolutely; but from them, and from various other considerations, I entertain little doubt, that though the exhalants of the skin pierce the epidermis, and come into contact with the external air, the mouths of the absorbents terminate under it, and are covered by it; and that while it remains unirritated and entire, no absorption of solid, liquid, or aëriform elastic fluid, takes place on the surface. In the instances that are supposed to favour the contrary opinion, it will be found, that the article absorbed is forced

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through

through the epidermis by mechanical pressure; or that the epidermis has been previously destroyed by injury or disease; or if sound, that the article applied to it is of an acrid nature, which first irritates and erodes this tegument, and then coming into contact with the mouths of the lymphatics under it, is of course absorbed.*

After these observations were committed to the press, my attention was drawn to the valuable me-

* The absorption of mercury, sulphur, oil of turpentine, and similiar substances, rubbed on the skin in the form of unguents, comes under the first of these descriptions; the absorption of the same substances, and in some rare instances of lead from the surface of wounds, and of herpetic, or other eruptions, under the second; the absorption of cantharides, aloes, garlic, &c. under the third. I am not ignorant of the experiments of Dr. Falconer and Mr. Abernethy, nor wanting in due respect to those gentlemen; but my own observations, supported by the experiments of M. Séguin, induce me to reject their conclusions. A probable source of error in the experiments supposed to afford a direct proof of cutaneous absorption is, that they were made on a small part of the body, e. g. the arm or leg, and not on the whole; though if the mouth and nostrils were left free for respiration, it would be as easy, and much more correct, to make experiments for this purpose, as well as for determining the quantity of exhalation, on the whole surface, as on a part. Dr. Marcand, who wrote expressly of the effects of the bath, has adopted the theory of absorption through the skin, without making any experiments.

moir on this subject read before the Academy of Sciences by M. Séguin, of which some account is given by Fourcroy, in the third volume of "*La Médecine éclairée par les Sciences physiques*, p. 232." The conclusions of M. Séguin correspond in every particular with those which I have already laid down, and being founded on a great variety of experiments, made for the express purpose of determining on the cutaneous absorption, they appear to me to give his inferences all the solidity of truth. The objection, that though there is no increase of weight in the bath, there may be an absorption, which is countervailed by the exhalation, M. Séguin found a satisfactory mode of invalidating. He dissolved in the water of the bath in which he made his experiments, substances which produce a specific effect when received into the system, by which their inhalation might be ascertained. He employed the oxygenated muriat of mercury in solution on a number of venereal patients, and while the epidermis was entire, he never perceived a single instance of salivation, or even of amendment of their complaints. But in cases where a considerable portion of epidermis was injured or destroyed, as in the itch, the specific effects of mercury on the system were produced. The experiments of M. Séguin are very numerous, and appear to be devised so as to meet every objection. He concludes from the whole, that while

the epidermis is entire, the absorbents of the surface take up neither water nor air, nor any substance diffused or dissolved in either.

Admitting that the inferences of the friend and coadjutor of Lavoisier are just, how are we to explain the refreshment and abatement of thirst, experienced by Mr. M. in the bath. Referring to what is said in pages 253, &c. on the nature of thirst, we may here shortly observe, that as liquids thrown into the stomach relax the exhalants of the surface, so doubtless this action is reciprocal; and that the exhalants of the surface being relaxed, in the warm bath, a relaxation in those of the stomach ensues, by which the sense of thirst is abated. In the cold, as well as in the hot state of intermittent fever, the thirst is great; but it is always lessened, and generally removed, on the accession of the sweating stage, though no liquid should have been swallowed.

Will this explanation, it may be asked, account for the benefit derived in accidents at sea, from immersing the body in water, or applying wet substances to its surface, when no drink can be procured? it will explain it in part perhaps, but not wholly.—In the torrid zone, where the greater part of these instances have occurred, the immersion of the body in water, or the application

cation of wet clothes to the skin, will keep the surface cool, by defending it against the direct rays of the sun, and thus prevent the debility produced by an accelerated circulation; it will also defend it, in whatever climate, from the wasting of evaporation, and thus be a means of prolonging the strength. This reasoning is supported by a fact which may be inferred from the circumstances I have mentioned, and which is established by the experiments of M. Séguin, that the body wastes much less in water than in air.

In the Buxton bath I found my weight very slightly diminished; but in the case of Mr. M. and in all our experiments on diabetic patients, there was neither diminution nor increase in the bath. In the experiments of M. Séguin, there was always some diminution in the bath, of whatever temperature, which he ascribes to the pulmonary perspiration. How is this difference accounted for? Though the cutaneous exhalation may have been stopped in Mr. M. during immersion, yet allowing the perspiration by the lungs to have gone on, this alone, supposing no absorption, ought to have produced a diminution of weight in the bath. To this it may be replied, that though no absorption through the skin takes place in the bath, that by the lungs goes on, while in the heat of 96° , the evaporation from the surface of the water, by
loading

loading with moisture the air inhaled, prevents its taking up the moisture in the lungs, and thus diminishes the pulmonary perspiration.—Against this source of error it is probable that M. Séguin guarded. It is in this way that the increase of the weight of the body in moist air, which is said sometimes to occur, and which has been supposed to prove a cutaneous absorption, may be accounted for. In moist air the absorption of the lungs from the atmosphere goes on unchanged, while the pulmonary, as well as the cutaneous perspiration, is diminished, and thus the increased heat of the body may be explained. On this point however more accurate experiments are required.

It was observed, that the discharge by urine alone, in the case of Mr. M. exceeded much in weight the waste of his whole body; and it cannot be doubted that the discharge by stool and perspiration exceeded the weight of the clysters. Thus it appears that the egesta exceeded the ingesta, in a proportion much greater than the waste of his body will explain. How, it may be said, is this accounted for, unless by cutaneous absorption? In the same way, it may be answered, by the action of the lungs.

That the oxygen inhaled in respiration from
the

the atmosphere, unites in part with the carbon of the blood, to form carbonic acid gas, and in part with the hydrogen, to form water, is generally admitted. In instances of inanition, we may suppose the proportion in which it unites to these different substances to be varied by the compensating powers of life; that the formation of water is greater than usual, and that of carbonic acid gas less. Thus a portion of the oxygen, which in the ordinary course of things is *exhaled in the form of an elastic vapour, being absorbed in the form of water*, will serve to retard the waste of the body, and to furnish the blood with the proportion of lymph, which affords a proper supply to the secretions of urine and perspiration. In the case of Mr. M. there was no appearance of that acrimony of the fluids which might naturally have been expected from the cessation of the supply of chyle, and which Haller describes as the constant effect of inanition.

This theory of the increased formation of water in the lungs has been employed by Dr. Rutherford of Edinburgh, to account for the vast discharge by the bladder in diabetes. It seems also to account for the phenomenon of the egesta exceeding the ingesta, so observed in that disease. It illustrates the doctrine of Séguin, in

in the memoir already quoted, of which Fourcroy gives an account in the following words—*Le diabète ne provient point de l'eau absorbée dans l'air par la peau, mais du reflux de celle qui ne peut pas être enlevée aux poumons par l'air trop chargé d'humidité*—a doctrine which appears to be defective, from not pointing out the source of the increased quantity of water in the lungs. Beautiful as the theory of Dr. Rutherford is, it cannot be considered as established in diabetes, or in inanition (to which I have extended it) till the air expired by patients under these diseases shall be examined. In the mean time I may observe, that the application of it to the case of Mr. M. accords with the usual œconomy of the living principle, which, in remedying the defect of particular organs, does not transfer their powers entire to other parts of the system, but modifies or increases the natural action of some other organ, by which that deficiency may be to a certain degree countervailed. Such an exertion of the living principle was evident in the increased powers of the rectum, which have already been described. These vicarious actions of the system are however defective, whether arising spontaneously, or from the assistance of art. No applications of food to the inner surface of the rectum can long supply the absence of food in the stomach, nor can the want of liquid in
that

that organ be long countervailed by applications of moisture to the surface, aided by the modified action of the lungs. After a certain time, though immersed in water to the chin, the thirst will prove intolerable without drink, as the shipwreck described in Chap. xv, will testify; and the sufferings of Tantalus,* which the theories of Dr. Marcard led him to deride, will be rated as highly by the naturalist as by the poet.

1798. The opinion given in the preceding pages

* Dr. Marcard, in the work quoted *p.* 189, after asserting the great inhalation of water by the skin in the bath, declares, that no naturalist will estimate the sufferings of Tantalus highly. It seems probable, that immersion in tepid water is more effectual in abating thirst, than immersion in cold water.

The account I have given of Dr. Rutherford's theory of diabetes, I derive from the valuable inaugural dissertation of Dr. Marcet of London, presented to me by the ingenious author.

The explanation offered by Dr. Marcet of the increased heat in diabetes, will be found I believe irrelevant.—In seven cases of this disease which I have examined, the heat previous to the medical treatment, was less, not greater than natural, notwithstanding the almost unanimous concurrence of authors in the contrary statement; a proof among many others of the inaccuracy of the accounts we have of temperature in disease.

having

having occasioned considerable discussion, I think it right to give an abstract from the report of M. Fourcroy, on the Memoir of M. Séguin not having been able to procure the Memoir itself.

M. Fourcroy begins with mentioning the circumstances which led M. Séguin to doubt the commonly received opinion, respecting cutaneous absorption, and to make new experiments on the subject. The Memoir read by him contains the result. “ Nous n'entrerons point,” says M. Fourcroy, “ dans le détail des nombreuses expériences qui y sont consignées, et que l'on “ affoiblirait par la rapidité nécessaire dans un “ rapport; nous nous contenterons de rappeler “ à l'Académie les principaux résultats qu'elles ont “ donnés. Le premier résultat tiré de trente-trois “ expériences faites sur lui-même, c'est que le “ corps n'augmente pas de poids dans le bain, “ qu'il perd moins dans l'eau que dans l'air, et que “ cette perte suit sur-tout la raison de la température de l'eau du bain; que la perte de poids “ dans l'eau à dix ou douze degrés (baromètre à “ vingt-huit pouces), est à celle dans l'air comme “ 6. 5 est à 17; qu'à quinze à dix-huit degrés de “ température, cette perte dans l'eau est à celle “ dans l'air comme 7, 5 est à 21, 7; que dans l'eau “ chaude à vingt-six ou vingt-huit degrés, elle est “ à celle dans l'air comme 13 est à 23. Deux autres

“ personnes ont offert des résultats différens dans
 “ ces pertes relatives, mais elles ont toujours moins
 “ perdu dans l'eau que dans l'air. M. Séguin
 “ attribue cette perte moindre à ce que la matière
 “ de la transpiration insensible n'est point exposée
 “ au contact de l'air qui doit la dissoudre dans l'état
 “ ordinaire. Il rend raison de la différence de
 “ ces pertes à diverses températures de l'air de la
 “ manière suivante. La perte de poids qu'on
 “ éprouve dans l'eau à dix ou douze degrés, est
 “ beaucoup plus foible que celle qui a lieu dans
 “ l'air, parce qu'il n'y a point de transpiration cu-
 “ tanée; il n'existe alors que la transpiration pul-
 “ monaire : celle qui se fait dans l'eau à dix-huit
 “ degrés est un peu plus foible que la seule trans-
 “ piration pulmonaire, parce qu'outre qu'il n'y a
 “ point de transpiration cutanée dans ce cas
 “ comme dans le précédent, l'air qui entre dans le
 “ poumon est chargé d'humidité, et ne dissout pas
 “ toute celle qui se dégage de ce viscère; enfin,
 “ la perte de poids qu'on fait dans un bain d'eau
 “ à vingt-huit degrés est plus considérable que
 “ celle qui est produite par la seule transpiration
 “ pulmonaire dans l'air à cette même température,
 “ parce qu'alors le corps perd, et par cette der-
 “ nière transpiration, et par la sueur qui sort des
 “ vaisseaux exhalés, en raison de l'augmentation
 “ de mouvement du cœur et des artères, qui,
 “ comme M. Séguin l'a prouvé dans son mémoire
 “ sur

“ sur la transpiration, est la seule cause de la trans-
 “ piration sensible, ou de la sueur. Mais, malgré
 “ la différence de ces trois résultats qui dépendent
 “ de la température de l'eau du bain, il n'est pas
 “ moins certain qu'il n'y a point augmentation du
 “ poids du corps par le bain, et qu'il y a seule-
 “ ment une perte moins forte que dans l'air, dé-
 “ pendante de l'absence de celui-ci et de la priva-
 “ tion de sa qualité dissolvante par rapport à la
 “ matière de la transpiration.

“ Les premières expériences dont nous ve-
 “ nons d'exposer les résultats généraux les plus
 “ importants, prouvoient bien que le corps n'aug-
 “ mente pas de poids dans le bain, mais elles ne
 “ décidoient point encore l'absorption ou la non
 “ absorption par la peau, car on pouvoit objecter
 “ à leur auteur que la perte moindre que dans
 “ l'air dépendoit de la portion d'eau absorbée par
 “ les vaisseaux inhalans. M. Séguin a pensé que,
 “ pour répondre à cette objection, il falloit faire
 “ baigner des individus dans des dissolutions de
 “ substances dont les effets sur l'économie animale
 “ fussent bien tranchans. Il a employé la disso-
 “ lution de muriate oxigéné de mercure, à des
 “ doses connues, en pédiluves, sur plusieurs ma-
 “ lades atteints de symptômes vénériens, et il a
 “ constamment observé que lorsque la peau étoit
 “ bien saine, et l'épiderme bien entier, il ne passoit
 “ pas

“ pas de sublimé corrosif dans leurs humeurs, ils
 “ n'éprouvoient aucun des accidens dus à ce sel,
 “ et aucune amélioration dans leurs maladies, tan-
 “ dis que dans le cas où l'épiderme étoit affecté
 “ et entamé, comme dans la gale, &c. ce sel pé-
 “ nétrait le corps et produisoit alors les effets qui
 “ en font reconnoître l'existence dans l'écono-
 “ mie animale.

“ Non content de ces expériences faites dans
 “ un hôpital sur plusieurs individus malades, M.
 “ Séguin a cru devoir les recommencer sur un
 “ sujet sain, les suivre avec une scrupuleuse exacti-
 “ tude, jusqu'à ce qu'il eût levé tous les doutes, et il
 “ s'est choisi lui-même pour sujet de ces nouvelles
 “ tentatives. En tenant à un grand nombre de re-
 “ prises différentes, pendant long-temps à chaque
 “ fois, une partie de son bras plongée dans une dis-
 “ solution connue de sublimé corrosif à différentes
 “ températures, et disposée dans un manchon de
 “ verre recouvert de taffetas gommé, de manière à
 “ ce qu'il n'y eût point d'évaporation sensible, le
 “ reste de son corps, excepté sa bouche, étant d'ail-
 “ leurs enfermé dans l'enveloppe imperméable
 “ de taffetas ciré, afin de pouvoir apprécier la
 “ transpiration pulmonaire comme il avoit ap-
 “ précié la perte dans l'air de la partie du
 “ bras plongée dans la dissolution, M. Séguin
 “ est parvenu à obtenir des résultats aussi singu-
 “ liers que nouveaux : nous ne rapporterons ici

“ que ceux qui ont trait à l'absorption par la peau.
 “ Quand la dissolution de deux gros de sublimé
 “ dans dix livres d'eau est à dix et à vingt-huit
 “ degrés de température, la quantité de ce sel
 “ dans le bain est très sensiblement la même après
 “ l'expérience qu'auparavant, et conséquemment
 “ il n'y en a pas d'absorbé; mais dans le même
 “ dissolution à dix-huit degrés, si la presque to-
 “ talité du bras tres-sain, et dont l'épiderme est
 “ bien entier, y reste plongée, il y a par heure 1,
 “ 2 grains de sublimé absorbé, quoique l'eau du
 “ bain qui tenoit cette portion de sublimé en dis-
 “ solution ne soit point elle-même absorbée. M.
 “ Séguin tire de ce singulier résultat l'induction
 “ que ce n'est pas par les vaisseaux lymphatiques
 “ que se fait cette absorption du sublimé, car ces
 “ vaisseaux absorberoient bien plus facilement
 “ l'eau, qui cependant n'éprouve pas de diminu-
 “ tion, mais par les vaisseaux exhalans. L'auteur
 “ explique ce phénomène d'une manière très-ingé-
 “ nieuse : il pense que ces vaisseaux exhalans resser-
 “ rés par des températures basses telles que douze,
 “ en évacuant des gouttelettes de sueur continuelles
 “ par un effort plus considérable du cœur et des
 “ artères produit par la température de vingt-huit
 “ degrés, et n'absorbant conséquemment aucune
 “ parcelle de sublimé dans les deux circonstances,
 “ se trouvent tellement disposés à la température
 “ de dix-huit degrés, que suffisamment dilatés pour
 “ que l'eau de la dissolution soit en simple contact

“ à leur extrémité avec l’humeur transpiratoire,
 “ sans être repoussée par l’écoulement de cette
 “ humeur, comme cela a lieu à vingt-huit degrés ;
 “ alors ce contact sans mouvement permet à l’hu-
 “ meur de la transpiration de dissoudre de proche
 “ en proche une partie du muriate oxigéné de mer-
 “ cure dissout dans le bain, de le partager avec
 “ sa première eau de dissolution, de se mettre avec
 “ elle dans un véritable état d’équilibre, comme
 “ cela a lieu dans toutes les dissolutions salines
 “ mêlées avec de l’eau pure. Ainsi, suivant lui,
 “ une dissolution saline à dix ou à vingt-huit de-
 “ grés étant mise en contact avec la peau humaine
 “ bien saine, il n’y a point d’absorption ni de la
 “ part de l’eau ni de la part du sel ; la même
 “ dissolution à dix-huit degrés, mise en contact
 “ avec la peau qui ne transpire point dans l’eau à
 “ cette température, permet à l’humeur de la
 “ transpiration de partager le sel de la dissolution
 “ jusqu’à l’équilibre de saturation et de le porter
 “ dans la circulation : cet effet n’a pas lieu lorsque
 “ les pores des vaisseaux exhalans sont resserrés
 “ par une température basse ou traversés par les
 “ courans de sueur qui repoussent la dissolution
 “ de sel. Les vaisseaux lymphatiques n’enlèvent
 “ ni solide, ni liquide, ni fluide élastique à la sur-
 “ face du corps ; les vaisseaux exhalans n’absorbent
 “ jamais ni solide insoluble, ni liquide, ni gaz,
 “ parce qu’ils sont toujours pleins de la liqueur
 “ transpirable.

“ transpirable. L'absorption qui n'a lieu que
 “ dans une certaine température, et qui tient à
 “ la tendance à l'équilibre entre un liquide non-
 “ saturé et un liquide saturé, est par cela même
 “ très-bornée.

“ M. Séguin, en poursuivant la description
 “ de ses expériences, confirme les assertions
 “ précédentes, par l'histoire de plusieurs malades
 “ vénériens, chez lesquels la dissolution de sub-
 “ limé employée en lotion n'a rien fait tant que
 “ l'épiderme n'a point été entamé; d'autres à
 “ qui le sublimé ainsi que le muriate d'ammo-
 “ niaque et de mercure ou le sel *alembroth*, appli-
 “ qués à sec, n'ont produit d'effets que lorsque la
 “ peau s'entamait par l'âcreté de ces sels; de
 “ quelques-uns qui n'ont éprouvé ni érosion, ni
 “ action conséquemment du muriate de mercure
 “ ou mercure doux appliqué sur la peau. Le
 “ tartrite d'antimoine et de potasse ou le tartre
 “ stibié, appliqué ainsi sur le ventre, a purgé après
 “ avoir produit des boutons dans le lieu de l'ap-
 “ plication; la gomme gutte, scammonée, ap-
 “ pliquées sur le ventre à sec, et recouvertes
 “ comme les matières précédentes d'un emplâtre
 “ agglutinatif à sa circonférence, n'ont fait naître
 “ aucun effet sensible. L'onguent mercuriel n'agit
 “ que par une friction qui fait pénétrer l'ox-
 “ ide de mercure sous l'épiderme, et le met dans
 “ le

“ le cas d'être absorbé ; un onguent fait avec le
 “ sublimé corrosif entame la peau, et devient un
 “ des moyens les plus prompts et les plus actifs
 “ de faire pénétrer du mercure très-oxidé dans le
 “ torrent de la circulation.

“ Il n'est pas nécessaire d'entrer ici dans de
 “ plus grands détails sur les expériences très-nom-
 “ breuses de M. Séguin, pour faire concevoir les
 “ résultats généraux qu'elles lui ont fournis et
 “ qu'on peut réduire aux suivans : 1°. Les
 “ vaisseaux absorbans n'absorbent dans aucun cas
 “ ni l'eau, ni l'air, ni les matières qui y sont mê-
 “ lées ou dissoutes, 2°. l'épiderme qui les re-
 “ couvre exactement dans l'état sain, les empêche
 “ absolument de faire cette fonction, et ils ne l'ex-
 “ ercent qu'au-dessous de cette croûte ; 3°. Les
 “ matières dissolubles sont peu-a-peu enlevées à
 “ l'eau qui les dissout par l'humour de la transpira-
 “ tion placée à l'extrémité des vaisseaux exhalans,
 “ lorsque cette humeur ne coule point en torrent
 “ comme dans la sueur, ou lorsque les vaisseaux
 “ ne sont point resserrés comme par une tem-
 “ pérature trop basse ; 4°. les matières liquides
 “ ou fluides élastiques ne sont point admises dans
 “ les vaisseaux exhalans, toujours pleins de l'hu-
 “ meur transpiratoire qui y séjourne ou qui y est
 “ dans un mouvement inverse à celui de l'absorp-
 “ tion ; 5°. les matières caustiques sèches ne sont
 “ absorbées

“ absorbées qu’après avoir détruit et corrodé l’épi-
 “ derme ; 6°. les matières sèches non solubles ne
 “ peuvent passer dans le système lymphatique,
 “ que lorsque par une friction plus ou moins forte
 “ on les a fait pénétrer à travers les mailles et les
 “ pores de l’épiderme, jusqu’à l’espace où s’ouvrent
 “ les bouches des vaisseaux absorbans.

“ A ces énoncés, qui résultent immédiate-
 “ ment des expériences indiquées, M. Séguin en
 “ ajoute d’également importants, qui ne sont que
 “ des conséquences nécessaires des premières et
 “ dont nous exposerons ici les principaux : 1°.
 “ les maladies épidémiques se contractent par la
 “ voie de la respiration, et les miasmes dissous dans
 “ l’air déposé dans les poumons sont absorbés par
 “ les vaisseaux absorbans de ces viscères qui, dé-
 “ pourvus d’épiderme, jouissent d’une force ab-
 “ sorbante très-entière ; 2°. le diabète ne provient
 “ point de l’eau absorbée dans l’air par la peau,
 “ mais du reflux de celle qui ne peut pas être
 “ enlevée aux poumons par l’air trop chargé
 “ d’humidité ; 3°. les amas d’eau ou les diverses
 “ espèces d’hydropisies ne dépendent que de la
 “ différence d’action entre les vaisseaux absorbans
 “ et les vaisseaux exhalans ; 4°. l’absorption com-
 “ mune dans les absorbans par le vuide qui y
 “ est produit, soit par la diminution de pression,
 “ soit par celle des stimulus, leur structure val-
 vulaire

“ vulgaire interne, détermine le mouvement des
 “ fluides de leur extrémité vers le système des
 “ vaisseaux sanguins; 5°. les matières âcres
 “ et stimulantes, en faisant contracter les vais-
 “ seaux, arrêtent l’absorption; l’affinité des sub-
 “ stances à absorber avec les vaisseaux absorbans
 “ détermine également cette fonction; telle est
 “ la différence d’action des matières nourrissantes
 “ et des purgatives, par rapport au système des
 “ vaisseaux absorbans abdominaux; 6°. enfin
 “ les virus contraires pénètrent par les poumons;
 “ l’épiderme est un rempart qu’ils ne peuvent
 “ franchir dans l’état sain et dans l’intégrité par-
 “ faite de ce tissu, et ils ne peuvent pas être
 “ absorbés par la peau.”

La Médecine éclairée par les Sciences
Physiques, vol. iii. p. 234.

I think it unnecessary to make any obser-
 vations on this extract, which will convince every
 candid mind that our conclusions respecting cu-
 taneous absorption have been hasty, if not wholly
 erroneous.

1803. As the inquiry respecting the absorp-
 tion of the surface of the body is not less impor-
 tant than curious, and as this incidental notice
 of it has excited considerable attention, I shall

give some account of the experiments on this subject, published at Philadelphia, in 1800, by Dr. Baptiste Clement Rousseau, of the island of Hispaniola.

The substances employed by Dr. Rousseau, were, spirit of turpentine, musk, garlic, and camphor; and these were selected, because on absorption, they produced an effect on the system which is obvious and easily ascertained. He details his experiments, however, with spirit of turpentine only, those made with the other substances corresponding with them entirely.

To ascertain the effects of spirit of turpentine when absorbed, Dr. Rousseau swallowed a few drops diluted with sugar and water. The effect was as he expected—in a little while his urine had a strong smell of violets. This effect always follows the introduction of spirit of turpentine into the system through whatever channel, and is, as Dr. R. believes, peculiar to that substance. In order to ascertain the uniformity of this effect of the spirit of turpentine, he had it thrown up into his rectum, in the form of a clyster, mixed up with the yolk of an egg, and diluted with tepid water. The clyster was retained, and no doubt absorbed. In an hour, the urine smelt strongly of violets, and the breath was strongly impregnated

ed with the flavour of turpentine. He inhaled from a narrow necked bottle, containing spirit of turpentine, a dozen times, put the bottle away, and went out into the air. An hour afterwards, his breath smelt strongly of turpentine, and his urine had a strong violet flavour.

Desirous of trying how small a degree of inhalation would produce this effect, at another time he made one single full inspiration in the same manner, and put the bottle away. In the course of an hour, his urine was very sensibly affected with the violet flavour, as before.

He poured a phial of spirit of turpentine on a table in a close room, and walked backwards and forwards for half an hour. At the end of this time, on examining his urine, it had the peculiar violet flavour.

The absorbing power of the lungs, and the rapid effect produced by it on the circulating fluids, and even on the secretions, being manifest, in the experiments made by Dr. Rousseau to determine the question respecting the absorption of the surface of the body, great care was used to guard against the action of the lungs.

Having provided a long pipé which communicated

nicated with the external air, he breathed through it, cautiously excluding all communication with the air of the room in which he sat. He then poured out the same quantity of spirit of turpentine on the table as in the foregoing experiment, stripping off his clothes, and exposing the surface of his body to the emanation. He continued in this situation for two hours, but no effect was produced on his urine, either at the end of that time, or for the whole day afterwards.

He immersed his arm in spirit of turpentine for two hours, luting the vessel containing it to the skin, so that no vapour could escape; no effect was produced on his urine or respiration, though in the course of the experiment, the fingers became painful and inflamed, from the irritation of the spirit of turpentine.

He smeared his body all over several times with the spirit of turpentine, guarding against the inhalation of the vapour by the lungs, by breathing the external air through a tube, as before described. He continued this some time. The surface was irritated, and pulse quickened, but no effect was produced on the urine.

These experiments were frequently repeated and with the same result, whether the spirit
turpentin

turpentine was employed, or camphor, garlic, or musk, as has already been mentioned.

These results have led Dr. Rousseau to doubt of the absorption by the skin, even in cases where friction is employed, as in the inunction of mercurial ointment. In the heat of the surface of the body, he thinks that mercurial vapours may arise from it, and be absorbed by the lungs, thus producing the specific effects of mercury on the system. In support of this supposition, he mentions the case of M. Achard, as given in the *Journal de Physique*, for October, 1782, who having left a dish, containing 20lbs. of mercury, over a furnace that was daily heated, and being himself exposed to the vapours, experienced a salivation at the end of a few days, as did two other persons who had been exposed in the same manner. I am not however disposed to agree with Dr. Rousseau in this opinion. I think with M. Seguin, that the oxyd of mercury is, by the mechanical action of the friction, made to penetrate the epidermis, and to come in contact with the mouths of the absorbents, which lie under its surface. The same seems to be the case in the inunction of opiate ointment, by which the specific effects of opium may certainly be produced on the system. I am also of opinion, that there are parts of the epidermis so thin, and of such
extreme

extreme sensibility, as to admit of absorption through them without the aid of friction. I speak of the glans penis, and of the lips. A melancholy proof was afforded me of this circumstance, in the case which I have mentioned, (p. 179) where hydrophobia proceeded from the saliva of a rabid dog applied to the lips. I have also been inclined to believe, that in those cases, where the naked body is immersed for a considerable time, in vapour, or liquid, of a nature that stimulates the epidermis, though it may not destroy it—as for instance in the nitric acid bath—that absorption may take place; but the extreme facility with which the lungs absorb, lead me to doubt the inference from the experiments which supported that conclusion. And in regard to the experiments, which have been supposed to prove the absorption of the gases by the skin, I fairly acknowledge that I doubt them entirely. On the whole, the principal function performed by the surface of the body is perspiration or transpiration, and the principal use of this function is to regulate the animal heat. Under this point of view, which has been hitherto almost wholly overlooked by physiologists, perspiration assumes its true character, and becomes an object of the first importance both in health and disease.

CHAP. XIX.

Population of Liverpool. Prevalence of fever among the poor. Benefit derived from the Manchester House of Recovery in contagious diseases. History of the establishment of the Liverpool House of Recovery. Extract from the report of the physicians respecting the means of preventing contagious diseases in Liverpool.

THE Typhus, or low contagious fever, prevails in all large cities and towns to a degree that those are not aware of, who have not turned their attention to the subject, or whose occupations do not lead them to mix with the labouring poor. In Liverpool it has been supposed that this disease is seldom to be met with, and it is certainly true, that the upper classes of the inhabitants are not often subject to its ravages. In the last twenty years, it has only once spread among the higher classes so as to occasion any general alarm; and when the extent to which it is constantly present among the poor, shall be proved by authentic documents, this circumstance will serve to demonstrate the narrow sphere of the contagion, and to shew how much it is within the limits of human power to lessen

lessen the frequency of the disease. Before we produce these documents, it will be proper to offer a few general observations.

The population of Liverpool, as taken by actual enumeration, amounted on the first day of January, 1790, to 57,961 persons. In the returns made to Parliament under the "Act for taking an account of the population of Great Britain," the number had risen in the beginning of 1801, to 80,759; being an increase of 22,798, in a little more than eleven years*. Of the

* The enumeration in 1789 was made by Mr. Makin Simmons, and published in Mr. Gore's Directory in the year 1790, but omitted in the later editions. It occupied him three months, viz. from October 13th, 1789, to January 13th, 1790, and was executed with great care and accuracy. Mr. Simmons gave the population of each street, separately, enumerating also the front houses, back houses or cottages, and the inhabited cellars, with the number of persons in each of these descriptions of habitation. The general result was as follows:

| | | | | |
|--|---------|---------------|--------|--------------|
| Front houses, - - - | 6540, | containing | 39,118 | Inhabitants |
| Back houses or cottages, | 1608, | containing | 7,955 | Inhabitants |
| Inhabited cellars, - - | 1728, | containing | 6,780 | Inhabitants |
| The Work house and other charities - - | 1,879 | Inhabitants | | |
| There were besides | } —717, | | | |
| empty houses, | | | | |
| Making in all, - - | 10,593 | houses, or if | | |
| the cellars be deducted, | 8,865 | houses, con- | | |
| taining - - - - - | | | 55,732 | resident In- |
| habitants. | | | | But |

the inhabitants of Liverpool it is ascertained, that about 9500 live in cellars under ground, and upwards of 9000 in back houses, which in general have an imperfect ventilation, especially in the new streets on the south side of the town, where a pernicious practice has been introduced, of building houses to be let to labourers, in small confined courts, which have a communication with the street by a narrow aperture, but no passage for the air through them. Among the inhabitants of the cellars, and of these back houses, the typhus is constantly present, and the number of persons under this disease that apply for medical assistance to the charitable institutions, the public will be astonished to hear, exceeds on an average, 3000 annually*.

The

But in estimating the population, one twenty-fifth part should be added for sailors, according to the rule laid down by Mr. Rickman, in his "Observations on the Results of the "Population Act." This will make an addition of 2229, making in all 57,961 persons.

The Return in 1801 was 11,446 houses inhabited, and 338 empty; in all 11784 houses, containing 77,653 resident inhabitants. To this add a twenty-fifth for sailors, and the whole number is 80,759 persons; being an increase of 2919 houses, and 22,798 inhabitants, in eleven years and a few months.

* From the first of January 1787, to the last of December 1796,

The public charitable institutions in Liverpool for the relief of the sick poor, are the Infirmary and the Dispensary. Fevers, by one of the original rules of the Infirmary, are excluded from that hospital, though this rule, as may be seen from the narrative with which this volume commences, has been occasionally departed from. But with a few exceptions, the care of the poor in fever falls on the Dispensary. Of this institution I was for several years one of the physicians, and such of my remarks respecting it as are not supported by authentic documents, are founded on personal observation. Unless in cases where they are removed into the Infirmary, the care of almost all the inhabitants of cellars, when sick, and of a great part of the inhabitants of the back houses or cottages, devolves on the physicians, surgeons, and apothecaries of the Dispensary. This de-

1796, inclusive, a space of ten years, it will appear by the table which follows, that 31,243 cases of fever were admitted on the books of the Dispensary alone, which on an average is 3124 yearly. If it be supposed that some cases may be denominated typhus by mistake, let it be considered, how many cases of this disease do not appear on the books of the Dispensary, though occurring among the poor, being attended by the surgeons and apothecaries of the benefit clubs to which they belong, &c. The list of diseases on the books of the Dispensary has not been kept with equal accuracy since that period, owing to the frequent changes in the apothecaries and secretaries since the death of Mr. Avison.

scription

scription of persons, amounting in all to about 18,500, ~~do~~ not appear to be healthy, the number of cases admitted annually on the books of the Dispensary, on the average of ten years, exceeding 13,000. In the commencement of the year 1780, a register of these cases was begun by Mr. Avison, late apothecary to this charity, and continued by him to the summer of 1797, when he terminated a life of spotless integrity, singular usefulness, and incessant exertion *. From this valuable record I have formed the following table.

* I insert the diseases of September, 1790, extracted from the Register, which at the same time will convey a specimen of our most prevailing diseases, and shew that inflammatory and symptomatic fevers are not included under the general title of fever.

September, 1790.

| | | | | | |
|-------------|-----|------------|----|-------------------|------|
| Febris | 310 | Catarrhus | 24 | Debilitas | 19 |
| Ophthalmia | 59 | Dysenteria | 2 | Hydrops | 8 |
| Erydanche | 13 | Cephalagia | 10 | Scrophula | 11 |
| Pneumonia | 28 | Apoplexia | | Rachitis | |
| Hepatatis | | Paralysis | 4 | Syphilis | 24 |
| Gastritis | | Dyspepsia | 21 | Icterus | 1 |
| Phthisis | 19 | Epilepsia | 1 | Dysuria | 5 |
| Rheumatisms | 52 | Asthma | 16 | Verues | 5 |
| Erysipelas | 8 | Pertussis | 11 | Psora | 142 |
| Variola | | Colica | 24 | Partus Difficilis | 1 |
| Eubeola | 29 | Cholera | 24 | Ustio | 14 |
| Scarlatina | | Diarrhœa | 24 | Ulcus | 61 |
| Phtha | 13 | Hysteria | 4 | Vulnus | 89 |
| Hæmorrhoids | 3 | Convulsio | 2 | | |
| Hæmorrhagia | 4 | Insania | 1 | | |
| | | | | | 1086 |

GENERAL

GENERAL TABLE.

Shewing the number of Patients admitted on the books of the Liverpool Dispensary, from the first of January, 1780, till the last of December, 1796, a space of seventeen years, distinguishing the numbers of each year, and of each month of every year.

| Year. | Jan. | Feb. | March. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Total. |
|-------|-------|-------|--------|--------|-------|-------|-------|-------|-------|-------|-------|-------|---------|
| 1780 | 890 | 888 | 1147 | 995 | 827 | 805 | 787 | 672 | 557 | 825 | 730 | 855 | 9978 |
| 1781 | 893 | 755 | 1074 | 1028 | 940 | 846 | 951 | 889 | 981 | 1212 | 1081 | 1099 | 11749 |
| 1782 | 1106 | 1041 | 1150 | 1243 | 811 | 1605 | 686 | 619 | 722 | 736 | 704 | 760 | 11183 |
| 1783 | 975 | 858 | 1199 | 1232 | 1096 | 1035 | 1064 | 1062 | 1050 | 1094 | 992 | 966 | 12623 |
| 1784 | 738 | 915 | 976 | 969 | 968 | 934 | 895 | 877 | 929 | 934 | 1055 | 1009 | 11199 |
| 1785 | 1092 | 1075 | 1124 | 1082 | 992 | 1015 | 856 | 850 | 1005 | 1057 | 882 | 885 | 11915 |
| 1786 | 982 | 760 | 1117 | 996 | 986 | 791 | 816 | 1010 | 930 | 895 | 858 | 959 | 11100 |
| 1787 | 1092 | 881 | 1060 | 1015 | 1041 | 964 | 820 | 821 | 868 | 848 | 958 | 900 | 11268 |
| 1788 | 1022 | 960 | 882 | 1002 | 944 | 903 | 916 | 1115 | 1009 | 927 | 951 | 1140 | 11771 |
| 1789 | 1415 | 1053 | 1044 | 1153 | 1113 | 1070 | 1123 | 930 | 1017 | 908 | 982 | 1095 | 12963 |
| 1790 | 947 | 1052 | 1361 | 1123 | 1152 | 1098 | 1039 | 1001 | 1093 | 1175 | 1162 | 1034 | 13237 |
| 1791 | 1223 | 1126 | 1323 | 1118 | 1176 | 1166 | 1091 | 1112 | 1241 | 1257 | 1274 | 1093 | 14200 |
| 1792 | 1307 | 1302 | 1427 | 1277 | 1159 | 1353 | 1106 | 1013 | 985 | 1342 | 1143 | 1030 | 14444 |
| 1793 | 1088 | 1037 | 1188 | 1228 | 1247 | 1065 | 1077 | 1065 | 1128 | 1335 | 1215 | 1159 | 13832 |
| 1794 | 1051 | 1175 | 1170 | 1138 | 1112 | 1105 | 1066 | 1089 | 1160 | 1032 | 1306 | 1273 | 13667 |
| 1795 | 1298 | 1369 | 2003 | 1171 | 1209 | 1151 | 1266 | 945 | 1064 | 1259 | 1300 | 1200 | 15235 |
| 1796 | 1120 | 1141 | 1230 | 1402 | 1030 | 988 | 948 | 918 | 934 | 1087 | 967 | 1236 | 13001 |
| | 18239 | 17388 | 20475 | 19172 | 17803 | 17894 | 16507 | 15988 | 16673 | 17913 | 17560 | 17693 | 213,305 |

By the foregoing table it appears, that the number of patients admitted under the care of the Dispensary annually, taking the average of the seventeen years preceding 1797, is 12,548. But if the ten years only preceding 1797, be taken, the average number will be found to be 13,355. It appears that the greatest number of patients admitted in any one year up to 1797, was in 1795, amounting to 15,235, exceeding the average of the last ten years by 1880; and that the smallest number admitted for the seven years up to 1797, was in 1796, falling short of that average by 354, and of the number admitted in 1795, by 2234. When it is considered that the difference between these two years falls chiefly in the winter and spring months; that the winter and spring of 1795 were the coldest, and those of 1796 the mildest, experienced in our climate for a long series of years; these striking facts will corroborate the statements of Dr. Heberden, in proving that the common opinion, which supposes a mild winter in our island to be unhealthy, and a severe winter to be favourable to health, is, in both particulars the reverse of the truth. It appears also, that though the number of patients in the winter months of 1794-5, greatly exceeded the general average, yet it was in February, and more especially in March, that the excess rose so very high; on this last month the astonishing number of 2003

1

patients

patients having been admitted. In the three preceding months the cold had been intense, but in March the weather became warmer, uncertain, and stormy. Hence it appears, that though intense cold is directly unfavourable to the health of our poor, yet that it is in the transition from this intense cold to warmer weather, that their constitutions sustain the severest shock; a position that is still more fully supported by the decisive evidence, that the month of March is the most unhealthy of the year, and next to it the month of April, taking the average of seventeen years, together. On the same evidence we may state the succession of the months in respect to their unhealthiness in the following order—*March—April—January—October—May—December—November—February—June—September—July and August.** But though on the whole summer and autumn are more healthy than winter and spring, the difference is perhaps less than might have been expected; and the difference even between the two months that forms the extremes, is not very great. In the seventeen years recorded in the

* It appears that June stands in the table higher than May or December, but this arises from the vast number admitted in June 1782, when the influenza was prevalent, and was confined nearly to that month. Allowance being made for this, June will stand as inserted.

table, the average number of patients for each month of the whole is 1045; the average number in the month of March is 1204; in August 940. These observations however are strictly applicable only to a particular description of persons in a particular situation—to the poor of Liverpool—and without the corroboration of other documents, must not be extended to the general mass of society, or to the kingdom at large.

I proceed to give, from the same authentic register, the table of typhus, the more immediate object of our inquiries.

A a

A TABLE,

A TABLE,
Shewing the number of Fevers admitted on the books of the Liverpool Dispensary, from the first of January, and 1780, to the last of December, 1796, inclusive; distinguishing the numbers admitted every year, and each month of every year.

| Year. | Jan. | Feb. | March. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Total. |
|-------|------|------|--------|--------|------|-------|-------|------|-------|------|------|------|--------|
| 1780 | 150 | 125 | 179 | 173 | 168 | 183 | 191 | 150 | 129 | 186 | 150 | 133 | 1917 |
| 1781 | 130 | 146 | 180 | 200 | 187 | 154 | 157 | 127 | 167 | 234 | 208 | 223 | 2113 |
| 1782 | 268 | 265 | 231 | 292 | 148 | 159 | 120 | 140 | 143 | 182 | 150 | 158 | 2256 |
| 1783 | 210 | 158 | 184 | 207 | 222 | 212 | 286 | 227 | 265 | 316 | 257 | 273 | 2817 |
| 1784 | 170 | 194 | 245 | 247 | 232 | 225 | 270 | 230 | 266 | 247 | 369 | 297 | 2992 |
| 1785 | 285 | 268 | 296 | 294 | 219 | 187 | 173 | 180 | 186 | 250 | 244 | 182 | 2764 |
| 1786 | 191 | 166 | 216 | 244 | 202 | 155 | 159 | 188 | 169 | 211 | 167 | 197 | 2265 |
| 1787 | 256 | 209 | 301 | 234 | 313 | 356 | 255 | 192 | 218 | 234 | 283 | 326 | 3177 |
| 1788 | 236 | 174 | 213 | 255 | 233 | 245 | 271 | 311 | 258 | 341 | 315 | 295 | 3167 |
| 1789 | 319 | 176 | 338 | 323 | 391 | 205 | 184 | 162 | 212 | 214 | 204 | 208 | 2936 |
| 1790 | 176 | 248 | 337 | 294 | 281 | 247 | 343 | 270 | 310 | 340 | 355 | 269 | 3470 |
| 1791 | 253 | 247 | 277 | 230 | 233 | 240 | 266 | 248 | 300 | 344 | 335 | 371 | 3344 |
| 1792 | 359 | 361 | 269 | 278 | 261 | 237 | 236 | 223 | 211 | 330 | 212 | 174 | 3151 |
| 1793 | 174 | 209 | 221 | 259 | 257 | 334 | 199 | 197 | 338 | 305 | 224 | 228 | 2925 |
| 1794 | 157 | 330 | 383 | 280 | 337 | 305 | 291 | 245 | 303 | 290 | 258 | 326 | 3405 |
| 1795 | 152 | 265 | 546 | 204 | 234 | 230 | 248 | 159 | 196 | 239 | 317 | 180 | 2970 |
| 1796 | 197 | 161 | 266 | 242 | 288 | 176 | 203 | 182 | 254 | 329 | 153 | 247 | 2698 |
| | | | 4682 | 4256 | 4206 | 3850 | 3852 | 3431 | 3925 | 4592 | 4201 | 4087 | 48367 |

Thus it appears, that of 213,305 patients admitted in the seventeen years preceding 1797, at the Dispensary, 48,367 have laboured under typhus; nearly a fourth of the whole; an immense proportion! It is curious to observe, that March takes the lead in respect to the prevalence of fever, as well as of diseases in general, and that August, as before, brings up the rear. It appears also, that the month of March 1795, which exceeded every other month in the production of diseases in general, exceeds in a still greater proportion in the production of fever; no less than 546 patients having been admitted in that month under this single disease. The *year* 1795 has not however the same unfortunate pre-eminence here as in the table of general disease, being surpassed in the number of fevers by several others. But the year 1796 enjoys a similar distinction as formerly, having produced fewer fevers than any other of the last ten, and 545 fewer than the average of the whole. Taking the seventeen years in the table, the average number of fevers annually is 2845, and the average number monthly is 237. The average number of March rises to 275, and of August, sinks to 202. On the whole the prevalence of fever is greater, and the influence of seasons upon it less, than might have been expected.

For the ten years preceding 1797, there were, on an average, 119 patients ill of fever constantly on the books of the Dispensary ; or if the seventeen years preceding 1797 be taken, the constant average number will be 109. Of convalescents, unfit for labour, the average number will be nearly as great. Thus, in Liverpool, 240 of the poor may be considered as constantly rendered incapable of earning their subsistence by this single disease, and as the poor seldom lay up any part of their earnings for a season of sickness, the expence of their maintenance must, in one form or other, fall on the public. If we take this as low as 10l. for each, it will amount to 2400l. annually.*

It would afford me particular pleasure to introduce authentic documents of the success attending the treatment of the typhus by my brethren of

* These calculations will not be expected to be minutely correct, but I apprehend they will in general be found under the truth. The average number of fevers annually for the last ten years, is 3124, and the mean duration of each fever is supposed to be fourteen days ; $\frac{3124 \times 14}{365} = 119.8$ daily. If the average of the last seventeen years be taken, this will give 2845 annually, which produces on the same principles 109 daily. I calculate that the period of convalescence after each fever is fourteen days, or upwards, which gives the same number of convalescents as of patients in fever, deducting those who may die, a much smaller proportion than could well be supposed.

the Dispensary ; but the valuable register which furnished materials for the preceding tables, does not mark the issue of the particular cases. In general however it may be asserted, that where the assistance of the Dispensary is resorted to in time, even under the disadvantage of being attended in confined and dirty habitations, a great proportion of cases terminate favourably. The general treatment of fever at the Dispensary, consists in giving antimonial emetics in the first instance, and afterwards bark, opium, and wine. Nourishing food is occasionally administered. Ablution with cold water has been used less than could be wished, and indeed it is in general difficult to employ it in the obscure cellars in which this disease is usually found. The fever that prevails among our poor is remarkably uniform : it is the pure typhus, to which the cordial treatment can be applied with safety, and to a great extent. Whereas among the higher classes, fever is often attended with inflammatory symptoms in the first instance, sometimes with pneumonic symptoms through a considerable part of the disease ; and in such cases the indications of practice being contradictory, success is much less certain.

Though the cure of this disease is a principal object of our charitable institutions in Liverpool, it is to be lamented that hitherto little or nothing has been done for its prevention ; although it may
be

be confidently asserted, that this object, great as it is, is not beyond the reach of human power.— This infection arises from a want of cleanliness, and ventilation; and its influence is promoted by damp, fatigue, sorrow, and hunger. When the subject shall be seriously taken up by the public, the means of securing cleanliness and ventilation, even in the greater part of our subterraneous dwellings, may be clearly pointed out; and though the secondary causes, if I may so call them, seem less capable of being removed, yet much may be done to alleviate them, when a comprehensive inquiry into the condition and wants of the poor, (the most pressing of all objects in the eye of enlightened patriotism) shall engage the attention of the legislature.*

Important

* The want of a diet sufficiently nutritious, is doubtless one of the causes that promote the typhus, and other diseases, among our poor. This does not seem to arise in general from the price of their labour being inadequate to furnish such a diet, but from their ignorance in the most advantageous modes of cookery, and still more their indulging in articles that consume their means without adding to their sustenance. In the eighteen hundred cellars in Liverpool, there are many in which animal food is not tasted more than once a week; but there are very few in which tea is not drunk daily; it is often indeed drunk twice a day. The money spent on tea is worse than wasted. It is not only diverted to an article that furnishes no nutrition, but to one that debilitates.

Important information in regard to the means of preventing the generation and progress of contagion, may be found in the writings of physicians of our own country and neighbourhood; of Dr. Percival and Dr. Ferriar of Manchester, of Dr. Campbell of Lancaster, of Dr. Haygarth of Bath, and Dr. Clark of Newcastle. The persevering exertions of Dr. Percival, Dr. Ferriar, and the other gentlemen of the faculty attending the Manchester Infirmary, have produced an institution in that town, which deserves to be imitated in every large city and town in the kingdom; a house of reception, or as it justly called, a House of Recovery for the poor, when labouring under fever. Into this asylum, on the first notice of disease, the patient is immediately removed, and proper methods taken to purify his habitation. By these means the contagion is extinguished in its birth, and the patient himself being removed to a situation where all the resources of art can be employed with advantage

tates the empty stomach, and incapacitates for labour. Hence the vast number of dyspeptic complaints among our patients at the public charities, which are almost all to be traced to the use of tea or spirits, often indeed assisted by depression of mind. At the Infirmary and Dispensary together, this class of patients exceeds five hundred annually. The great majority are females.

for

for his benefit, and removed at a period of the disease when these resources are of great avail, is very generally restored in health to his family and to the community. The benefits of this admirable institution in Manchester, no longer rest on the basis of theory; they are established on the immoveable foundation of fact and experience. The prevalence of fever in the town is diminished to a degree that has exceeded all rational expectation; and the fears entertained, that the House of Recovery might spread the contagion through the streets in its vicinity, are found to be entirely groundless.*

The

* The following facts are extracted from the Report of the *Board of Health*, dated *Manchester, May 26th, 1797*. Since the establishment of the *House of Recovery*, the whole number of home patients (i. e. patients requiring attendance at their own habitations) is reduced *one half*; but the number of fever cases, in a far greater proportion. In January 1796, 226 cases of fever were admitted at the Infirmary of Manchester, but in January 1797, only 57. Fears had been entertained that the fever might spread from the House of Recovery into the neighbouring buildings, and so through the town, and this had given rise to a considerable opposition. The streets in the immediate vicinity of the House of Recovery, are Silver-Street, Portland-Street, &c. In the first eight months *after* the establishment of the House of Recovery, the number of cases of fever admitted on the books of the Infirmary from these streets was 25. In the eight months of the preceding

The utility of the House of Recovery in Manchester was still more completely ascertained in the course of the summer of 1797.—“ Within these “ last four months (I copy a paragraph in a letter from Dr. Ferriar in the month of September of that year)

preceding year, viz. from the 20th of September, 1795, to the 20th of May 1796, the same streets furnished 267 cases of fever. From the 20th of September 1794 to the 20th of May 1795, they furnished 389 cases; and from the 20th of Sept. 1793 to the 20th of May 1794, 400 cases. Facts such as these, require no comment; they have made a forcible impression in Manchester, as might be expected, and the Report I quote contains public testimonies in favour of this excellent Institution from the *Board of the Infirmary*, the *Stranger's Friend Society*, and *The Special Board of the Poor*. The total number of fever-cases received into the House of Recovery from its being opened, the 19th of May, 1796, to the 31st of May, 1797, is . 371

| | | |
|--------------------------------|-----------|-----|
| Of these are discharged cured, | | 324 |
| Dead, | | 40 |
| Remaining, | | 7 |

—371.

The benefit of this institution is not to be calculated by the number of persons cured. Every single removal into the House of Recovery, probably prevents on an average two or three cases of the disease. As the institution goes on, it may be confidently predicted, that the proportion of cures will be still greater than here stated, because experience will induce the poor to apply for relief in the early stages of fever when the power of medicine may be exerted with the greatest

year) we have been threatened with the return of
 “ an epidemic fever, and dangerous cases of ty-
 “ phus have appeared in every quarter of the
 “ town; yet by selecting those patients who were
 “ from their symptoms and situation, most likely to
 “ communicate infection, and by removing them
 “ into the fever-ward, the weekly list of home
 “ patients has been prevented from increasing
 “ much beyond their usual number. Our house
 “ of recovery, which contains accommodations
 “ for twenty-eight patients, is sufficient for Man-
 “ chester at present. In the event of a peace,
 “ and the influx of new inhabitants which it
 “ must occasion, it may be necessary to provide
 “ accommodation for a greater number. But I
 “ think fever-wards capable of containing fifty

est advantage. I have the authority of Dr. Ferriar, for say-
 ing, that since the 31st of May to the 25th of September,
 two persons only have died in the House of Recovery.

1803. The advantages at first derived from the House
 of Recovery at Manchester, have not continued in an equal
 degree, owing to the difficulty of enforcing cleanliness and
 ventilation in the habitations of the poor, difficulties parti-
 cularly great in Manchester, where such sudden fluctuations
 occur in the numbers of the lower classes. The importance
 of the House of Recovery is however universally admitted
 there, and a new building to contain 100 beds is now pro-
 jected for that institution.

“ patients,

patients, with separate rooms for the reception
 “of cases of *Scarlatina Anginosa*, *Measles*, or
 “*Small Pox*, would answer every purpose^f here
 “in times of the greatest emergency, even sup-
 “posing a very considerable increase of popu-
 “lation.”

“Till the latter end of the year 1787, there was no place for the reception of fever, or any other contagious disease, in Liverpool. At that time the fever broke out in the Infirmary, described in the sixth page of this volume, and two wards being fitted up for the patients infected, one for each sex, these were afterwards occupied by such cases of fever as occasionally presented themselves. The reception of fever into these wards continued for five years and one month, during which time 232 cases of fever passed through them. The wards were small, and under ground; in every respect inconveniently situated. They held at most eight beds each, but were properly adapted to six beds only. But they admitted of cleanliness and ventilation, and it was there that an opportunity was afforded of employing the affusion of cold water; of remarking its effects; and of establishing the rules that ought to regulate its application.

From the first however, the admission of
 1 fever

fever into the Infirmary, (which was contrary to one of the original rules of the institution) was objected to by some of the friends of that charity. They allowed the benefit arising from a receptacle for fever, but they contended that the Infirmary was not adapted to that purpose; and that the attempt to render it an hospital for fever, the wards already mentioned being all that could be allotted to that disease, did not afford the relief required, while it obstructed the original purposes of the institution. The want of an asylum for this disease elsewhere overcame these objections. But about the beginning of 1793, a contagious fever having prevailed in the Workhouse, two spacious wards were fitted up in that building by the Parish Committee, for contagious diseases; and an arrangement having been made for the reception of such cases of fever as presented themselves at the Infirmary, into the wards of the work-house, from that time the admission of fever into the Infirmary has ceased.

Contagious diseases, and more particularly fevers, have in general been excluded from the hospitals of England, those of London perhaps excepted; but the evidence of our own Infirmary and Workhouse, of the Chester Infirmary, and of various similar institutions, proves, that under proper regulations they may be admitted under the same

same roof with other diseases, without danger of the infection spreading through the building. It is not however to be denied, that the attendance and the arrangement requisite for patients under fever, render it a matter of convenience and propriety that they should be received into a separate building; and it is a point capable of demonstration, that hospitals for such diseases stand pre-eminent in point of utility over all other hospitals, those for accidents that require the immediate assistance of surgery alone perhaps excepted. The benefit derived from hospitals in other cases consists in removing disease, and is confined to the patient himself; but in cases of contagion, the evils prevented are much greater than those remedied, and the benefit is by this means extended from the patient himself to the circle by which he is surrounded. The establishment of such hospitals was first suggested by tracing the infection which propagates fever, to its origin, and ascertaining the power of ventilation and cleanliness in preventing and in alleviating the disease. The arguments for such hospitals are strengthened by the improved methods of destroying contagion; and if I do not greatly deceive myself, they are still farther strengthened by the success of that mode of practice in fever, which it is the chief object of this publication to explain and to establish. A vigilant

lant exercise of all means of prevention, and of cure, might indeed in a short period supersede the use of hospitals, by extinguishing the disease; a prospect in which the philanthropist might more safely indulge, if he could calculate with the same confidence on the wisdom as on the power of his species.

The wards for fever at the workhouse, have in general secured that immense hospital, often containing 1200 persons, from the spreading of fever, to which, previous to their establishment, it was perpetually liable; and though they are in the very centre of the building, and cannot be entered but through the common stair-case, yet no single instance has occurred of the contagion extending from them into the other parts of the house. They have also relieved the Infirmary from the necessity of admitting those miserable wretches who, under the influence of fever, (perhaps caught on ship-board) and refused admittance into private lodging, were brought up to that hospital, where, if not received, they might have perished in the streets. But they have not been of sufficient size to answer the purpose of a general House of Reception for the poor, labouring under fever throughout the town, nor has their power of accommodation been employed to its full extent with

with this view.* The physicians of the Dispensary, who attend the Workhouse, have authority to remove patients on the first appearance of fever, from any part of the Workhouse itself, into these wards; but they have not the same authority to remove them from the town. The delay occasioned by the forms to be passed through in obtaining the admission of these patients has very generally prevented the attempt being made, and sometimes where it has been made and succeeded, has occasioned the relief to come too late.

In consequence of considerations of this kind, I made a motion at the annual vestry, in March 1796, that a set of fever-wards should be constructed, at the expence of the parish, for the general use of the poor, on the plan of the Manchester House of Recovery. Four wards, capable of containing sixty beds, were stated to be sufficient for the town; it was proposed that they should be erected on the elevated and open ground

* These fever-wards contain eighteen beds very conveniently, and from the 20th of August 1793 to the 31st of August 1797, a space of four years, only 530 cases passed through them. They did not therefore on an average contain more than six fever-cases at a time. Of these 530 cases, 51 died, a small proportion, considering that 16 died in 24 hours after admission, and were in *articulo mortis* when received.

contiguous to the Workhouse, and be supported out of the parish rate. And it was asserted, that this would be a plan, not merely of charity, but of œconomy. The measure of establishing feverwards was unanimously agreed to, and a committee appointed to carry it into effect; but differences of opinion arose as to the execution of the plan, and all proceedings on the subject were suspended for several years. It has however been resumed, and is now nearly completed, in consequence of the circumstances about to be related.

So far as we can trust the bills of mortality published in Liverpool, and so far as we may consider the annual number of deaths as a fair criterion to judge by, the general health of the town does not appear to have been very materially affected at any one time during the last century, either by the prevalence of epidemic diseases, or by any other cause. The number of deaths bears a pretty fair proportion all along to the increasing population.* But in 1800, a sudden increase

* The most striking exception to this remark, appears in the year 1770, when the number of deaths amounted to 1562; being an increase of nearly 600 above the years which preceded and followed it. I use here and throughout, the general table of christenings, burials, and marriages, and the annual

increase in the proportion of deaths took place, which rose still higher in 1801, the whole number being in the first year 3080, and in the last 3768. For the two preceding years, the average annual number of deaths was 2406, and this is nearly the number of the year following, (1802) so that in 1800 the deaths exceeded by 678, and in 1801, by 1362, the usual number. So extraordinary an increase of mortality, arising as was evident from the prevalence of contagious diseases, excited considerable attention and alarm, and the motion being renewed by me in the annual vestry for the establishment of a House of Recovery without delay, the measure was unanimously agreed on: a specific levy of nine-pence in the pound being laid on the parishioners of Liverpool, for the express purpose of erecting this building, and providing a new cemetery for the parish poor. A plan for the building was given by the physicians of the Infirmary and Dispensary, and notwithstanding some unexpected delays, the house is

annual tables of the same kind, printed by Nevett, and published and distributed by the sextons, though I am afraid they are often deficient in accuracy. They differ considerably from the General Table published in the *Abstract of the Answers and Returns to the Population Act*, p. 19, said to be collected from the Register of the Town of Liverpool, in which the burials for 1770 are stated at 2,020.

now in great forwardness, and will be ready for the reception of patients in a few months. It is a spacious building, standing alone in a field by the Workhouse, from which it is distant about forty yards. It possesses every advantage of structure and situation, and will contain sixty, or on an emergency, ninety beds, of which one third, on a separate floor, are destined for convalescents. It is to be supported out of the poor-rate, under the management of a sub-committee, chosen out of the general parish committee appointed by the annual vestry, and an accurate account of its expenditure is to be kept and published annually. It is not doubted that this Institution will be found a measure not merely of policy and humanity, but of œconomy also*.

* In erecting a separate building, and forming a separate institution for the reception of fevers, the great number of persons requiring admission into this hospital was chiefly considered. For as I have already observed, it cannot be doubted, that in situations where these are less numerous, the reception of fevers may be safely combined under proper regulations with the usual objects of Infirmaries, as the curious, ample, and most important collection of documents on this subject, published by my friend Dr. Clark, of Newcastle, clearly demonstrates.—*See a Collection of Papers intended to promote an Institution for the cure and prevention of infectious diseases in Newcastle.*—Newcastle: printed by S. Hodgson, 1802.

That

That the Hospital for contagious diseases should have been erected by the parish of Liverpool, and that its annual expences are to be defrayed out of the poor-rate, are circumstances rather unusual in the history of such institutions. The application of the poor-rate to such purposes is however not only legal, but prudent and wise—of all the purposes indeed to which it can be applied, this is perhaps the only one which is no less strongly recommended by policy than by humanity.

I have purposely avoided entering into the detail of all the obstructions which this important measure encountered during the series of years in which it was under discussion, though in some points of view, such a detail might have been instructive. To the credit of the town it was in the end adopted almost unanimously, as it had from the first been unanimously recommended by the gentlemen of the faculty.

The effects of this institution will however be much limited, if measures are not combined with it for removing the causes from which contagious diseases derive their origin among our poor. Some attention has already been paid to this important object. In the beginning of the year 1802, the corporation of Li-

verpool, being about to apply to Parliament for powers to improve the streets and the police of the town, requested the physicians of the Infirmary and Dispensary to suggest to them "such alterations as might contribute to the health and comfort of the inhabitants," in order, that where necessary, they might include in the bill about to be brought into Parliament, the powers requisite to carry such alterations into effect. The physicians took this request into serious consideration, and presented a report of considerable extent, including a view of the causes of the uncommon sickliness of the two preceding years, and of the measures requisite to prevent its recurrence, and to remove the frequency of contagion in the habitations of the poor. To lessen as much as possible the contamination of the atmosphere, they recommended that lime should be prevented from being burnt within a certain distance of inhabited houses; that soaperies, tan-yards, and other offensive manufactories should in future be prevented from being established in the town; and where now established, and authorized by usage, that they should, whenever practicable, be purchased by the body-corporate, and the space they occupy converted to other purposes. The same recommendation they extended to slaughter-houses, and to all other offensive trades or manufactories. They recommended,

mended, that in all cases where fire-engines, or steam-engines are necessarily employed in the town or its vicinity, the burning of smoke should be enforced, as well as in all other practicable cases where large volumes of smoke are emitted.— They pointed out the necessity of enforcing cleanliness in the streets, to which end an improvement of the pavement was represented to be essential; and they particularly advised a general review of the common sewers, and an improvement of their structure, on the principles of a report on this particular subject, addressed by them to the Mayor and Magistrates, in 1788. They further advised, that effectual provision should be made for draining the grounds within the liberties, and particularly to the north of the town. “Repeated remonstrances (I quote the words of the report) have been made for the last twenty years, on the collections of standing water, including filth of every kind, which are suffered to remain in the district, which extends along the termination of the streets from St. Paul’s square to Byrom-street, and to which the low fevers, which in the autumnal months especially, infest these streets, are principally to be imputed. These remonstrances have been passed over, on the ground, as we are informed, that the proprietors of the lands will not agree to the plan necessary for draining them. It is not to be endured, that

that the health of the inhabitants should be perpetually endangered from such a cause, and we cannot conceive any subject more proper to be submitted to the wisdom of Parliament.

“ But in a more especial manner, we would direct the attention of the gentlemen of the town-council to the habitations of the poor, and in the first place to the inhabited cellars. The vast number of persons that occupy such dwellings, and the impossibility of finding other habitations, forbid us to hope that any recommendation for preventing them from being generally inhabited, could be attended with immediate effect. But a general survey should be made of these subterraneous dwellings, and such means adopted for promoting their salubrity as circumstances require and admit. Many of these cellars are double, and the apartments next the street, having access to the external air only by the narrow and oblique aperture that forms the door, and which is of course shut during the night; and the back apartment having access to light and air only by its communication with the apartment in front; under such circumstances the generation of disease, and especially of contagious fever, is almost a necessary consequence. In all such cases, a direct communication should be made with the external air, by opening a window in each apartment,

apartment, and where this is impossible, by introducing a tube, to the external orifice of which, an air-pump, of an easy and simple construction, may be applied when necessary, especially in cases of contagion, and the apartments be speedily and effectually ventilated. This would in a very considerable degree facilitate the means of counteracting contagious diseases, and of rendering the administration of medical assistance safe to those whose duty calls them to administer it. But, besides this, these cellars should, in all practicable cases, have a chimney in each apartment, and open into a back yard, where there is a necessary, and a supply of water; and no cellar should be inhabited, the ceiling of which is not three feet at least above the curb-stone round the door way, by which the communication with the external air may be rendered free and direct. Power should also be obtained, to oblige the owners of cellars to white-wash them, at a stated time in every year, and at all other times when they may have been visited by contagion; and likewise to prevent them from allowing these cellars to be inhabited till the plaster of the walls is perfectly dry.

“ In the application of these observations to cellars already inhabited, some modification must necessarily be allowed, but in regard to all houses
to

to be built in future, powers should be obtained to prevent the cellars from being inhabited at all, except they be constructed according to the plan just pointed out."

The Report here mentions a few places in which, from local circumstances, the cellars are particularly obnoxious, and advises, that as soon as possible, they should be emptied of their inhabitants, and filled up. It proceeds as follows :

" The habitations of the poor in the greater part of the small and narrow courts back from the streets, are equally objectionable in point of health as in the cellars. It is much to be lamented that such a form of building should have grown into general practice. * * *. Powers should be obtained for preventing ground from being occupied with buildings of this description in future, and the proprietors of courts already built, might be compelled to give them the ventilation of a thorough draught of air, which would in general be obtained with little difficulty. If any courts are permitted to be built in future, it should be on condition that they be made of a certain width ; that the entrance should not be through an arch-way ; that the houses should not be above two stories high ; and that the upper end of the court should be kept open

open.—Every court should have two or more necessities, according to its size, and a plentiful supply of water.

“ The Parish are now building an hospital for the reception of contagious diseases. The benefit of this will be greatly increased, if a power is obtained, enabling the magistrates, on the representation of two or more physicians, of the necessity of the measure, to remove the inhabitants of a dwelling declared to be infected into another habitation, till measures are employed for destroying this infection. For such a desirable object, it would be necessary to have a few dwellings kept for occasional and temporary use, in different parts of the town.

“ But after all it seems necessary to adopt some means for increasing the number, and improving the structure of the dwellings of the poor. It seems highly desirable to determine on a plan, both for the individual houses, and their arrangement as to each other, that may unite healthfulness and convenience, and serve, not only for present use, but for future example. Such a plan, when perfected, would contribute not merely to the healthfulness, but to the beauty and regularity of the town, while, at the same
C c time,

time, it would be of great advantage to the corporation estate. It would be favourable to industry and good morals. In the dwellings of the poor, which at present too frequently exhibit distress and confusion, might be introduced cleanliness and good order, and decency and comfort might be found in situations which too often excite sentiments of pity and disgust.

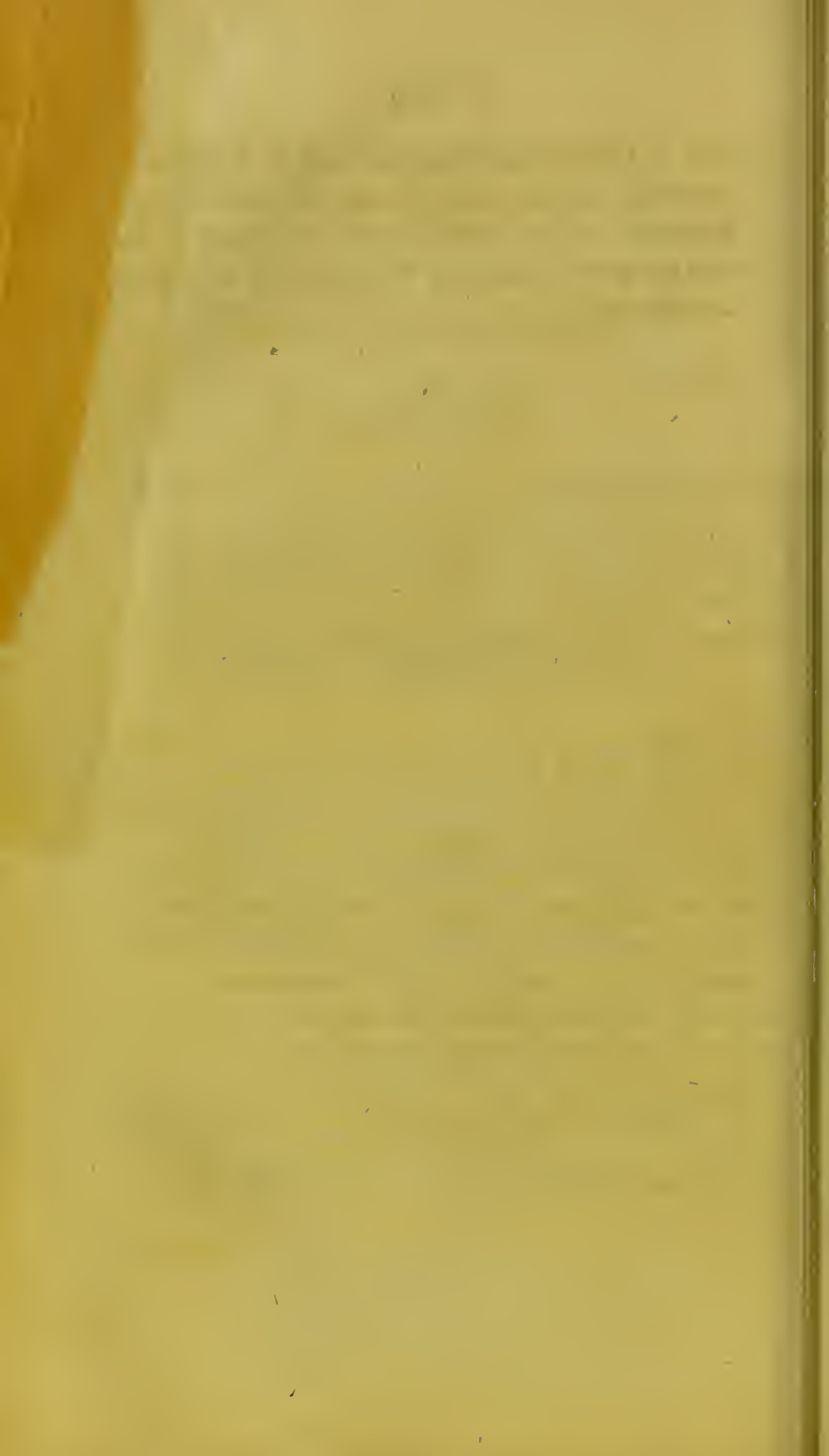
“ To such a change, other regulations would indeed be requisite, and particularly some means of diminishing the temptations to the use of intoxicating liquors, a great and increasing evil, but of a magnitude too serious to admit of its being on the present occasion fully discussed.”

Every attention was paid to this memorial by the gentlemen of the Common-Council of the Corporation, and in the draught of the Bill proposed to be brought into Parliament, such clauses were introduced as were necessary to enable them to carry into effect, the various regulations recommended to their notice. But the interests of different individuals being affected, not merely by these regulations, but by a variety of others proposed in the bill, difficulties have arisen in procuring that general assent, so desirable in all such applications to the legislature. The mea-

sure

sure is however in progress, and it is not to doubted, that by the exercise of candour and patience, every obstacle will be removed, and that the public good will triumph over all inferior considerations.

END OF THE FIRST VOLUME.



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V O L. I.

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MEDICAL REPORTS,

ON THE

EFFECTS OF WATER,

COLD AND WARM,

AS A REMEDY IN

FEVER AND FEBRILE DISEASES,

Whether applied to the Surface of the Body, or used Internally.

VOL. II.

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By JAMES CURRIE, M. D. F. R. S.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

*Intentiones operationum, quas proposuimus (ut arbitramur) verissimæ sunt, remedia
intentionibus fida. * * * Rem ipsam experimentum et comprobavit et promovebit.
* * * Opera consilii cujusque prudentioris, sunt effectu admiranda, ordine
quoque egregia, modis faciendi tanquam vulgaria.*

BACON. Historia vitæ et mortis.

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ADDITIONAL

ADDITIONAL REPORTS.

CHAP. I.

The Author's more recent experience of the use of the cold and tepid affusion.—Use of the Digitalis in inflammatory fevers.

IN the preceding volume is reprinted my former publication, with several additions, and a few corrections, suggested by the progress of my experience. Such additional observations as I have to make on the use of water as a remedy in fever and febrile diseases, I have, however, reserved for a separate volume, in which I shall include a few of the most striking testimonies in favour of this remedy, afforded by the experience of others.

In the six years which have elapsed since the publication of the second edition of the first volume,

I have invariably employed the affusion of water, cold and tepid, in the diseases pointed out in the original publication, and I have extended it to some others, of which I shall afterwards speak. Its success has equalled my expectations: I have nothing to detract from the accounts I have formerly given of its efficacy. I repeat—that used in the three first days of fever, the cold affusion very generally stops the disease—that the same happy effects sometimes follow its use on the fourth or even fifth day, but seldom later—that even in the subsequent stages, where the heat continues preternaturally great, and the skin dry, it is of great and manifest advantage, almost immediately relieving the most distressing symptoms, particularly restlessness and delirium, and conducting the disease to a safe and speedier issue.

The tepid affusion is, as I formerly observed, applicable to all the diseases to which the cold affusion is applicable, and possesses very considerable, though inferior efficacy. I find it however, very safe, easy of application, and in a high degree grateful, and I have extended it to almost the whole class of febrile diseases. In my practice the cold and tepid affusions are very often combined in the same disease. While the heat is great, the skin dry, and the vascular action strong, I use water perfectly cold; when these symptoms diminish,

diminish, I use it cool; and as they subside still farther, I make it tepid. The precise meaning of these terms is given in chap. x. vol. 1.

Though, where the fever is evidently of a contagious nature, I employ the cold affusion freely, notwithstanding the presence of pulmonary symptoms, as in the epidemic in the 30th regiment, mentioned in chap. iii. vol. 1. yet in anomalous cases, where these symptoms are not present, I have been cautious in using it. In such cases however, I use the tepid affusion freely, endeavouring to compensate for its weaker and more transient effect, by frequency of repetition. A great part of the febrile affections of children are of this class, and to these little innocents the tepid affusion is a blessed and an invaluable remedy. In fevers accompanied by, or originating in high local inflammation, which are generally attended by a great disposition to chilliness, I do not depend on or recommend the use of affusion, cold or tepid. Neither does my experience enable me to speak decidedly in favour of the use of cold water as a drink in such cases, though this is strongly recommended by Dr. Kinglake. It is a point on which I would not be understood to give an opinion.

It is to be regretted, that where fevers arise

singly in private practice, it often happens that before their nature is suspected, and a physician called in, the best season for using the cold affusion is past. In all such cases, however, ablu-
 tion may be resorted to with advantage, with water either warm, tepid, or cold, as the heat of the patient may direct; and by the method of affusion, or sponging of the surface, * as his strength may admit; the first being always to be preferred, unless in cases of great debility. By this practice, the patient himself is not only benefited, but the attendants are in a great measure secured against infection, if I may judge from a strict attention to this point, now of several years continuance. It is true an enquiry into this subject is not without difficulty. The period during which the infection of fever may lie dormant in the system without its being perceived in its effects is undecided. The observations of Dr. Haygarth on this subject are however of great weight, and indicate that this period extends to eight or ten days, or even more. Admitting then that the ablu-
 tion of a patient under fever may be a means of safety to the attendants, it cannot prevent infection from manifesting itself that has been received before the ablu-
 tion was per-

* For the precise meaning of these terms, see p. 73, vol. 1.
 formed;

formed; and if infection may lie eight or ten days dormant, fevers appearing within that space of time, after the first employment of ablution, ought to be referred to infection previously received. If the truth of these premises may be assumed, my experience testifies strongly in favour of the use of ablution, as a means of preventing the propagation of contagion. I have known a fever to spread after ablution had been repeatedly and effectually performed on the existing cases of the disease, but in every instance, I think, within ten days after this had begun to be practised. To arrive at certainty on this important point, will require numerous observations; but we cannot err in inferring, that among the means of preventing the propagation of contagion, general ablution of persons labouring under the contagious disease, is one of the most effectual.

Though a fever not ^{ing} ~~been~~ suspected may in the first instance be for many days overlooked, yet where its existence is once ascertained, vigilance will be excited, and future instances of its occurrence detected earlier. In such cases it is that the cold affusion appears to the greatest advantage. I might illustrate this by a great variety of examples, but the following shall suffice.

In the month of August, 1799, a warehouse-
 B 3 man

man, in the service of Messrs. Hodson and Carter of Liverpool, was affected with the low contagious fever, of which he died. I did not visit him and cannot mention the particulars of his case. The disease spread among several of the persons round him, and among others, Mr. Bourne, a clerk in the house, who had been frequently with the warehouse-man, caught the infection. He was attended by Mr. Ellison, by whom my assistance was requested on the 12th or 13th day of the disease. The situation of Mr. B. was at this time deplorable. He was sunk down in bed in the low delirium, insensible to surrounding objects. His pulse was feeble, and frequent almost beyond calculation; petechiæ covered his body; he was affected with diarrhœa, and passed his urine and stools involuntarily. I should have considered his case as desperate, had it not been that his respiration was tolerably free, that his deglutition, though impaired, was still equal to the purposes of nutrition, and that *his heat was steadily and considerably greater than natural.*

As his strength did not admit of his being taken out bed, the surface of his body was washed by means of towels dipped in cold vinegar and water. An anodyne absorbent mixture was directed to be given from time to time, to stop the diarrhœa and procure sleep. Yeast, to the quantity

tity of a pint daily, was also prescribed, with broth, and gruel mixed with wine, for his food. On the same day his mother arrived from a distance to take charge of her son, a woman of the strongest affections, but regulated by a corresponding strength of mind. She attended him night and day, forthwith, and implicitly followed our directions.

Mr. B. lay on his back, sometimes in a disturbed slumber, and sometimes with his eyes open, muttering to himself, and under the influence of subsultus tendinum. His heat, though always greater than natural, varied in degree from time to time, as often occurs on the approach of death, the exacerbations of heat being distinctly marked by a deep flush extending over his face and neck. On every appearance of this symptom, his mother was directed to repeat the ablution of his body with vinegar and water. The ablution was performed from eight to ten times in the twenty-four hours, and was always followed by abatement of febrile agitation, and generally by sleep. The other directions were followed strictly. In forty-eight hours a dawn of recollection appeared, and the involuntary discharges ceased. In eight days more his recovery was ascertained.

Mr. Hodson had also very humanely visited

the warehouse-man, and been frequent in his visits to Mr. Bourne. He was seized with fever, but did not at first suspect the nature of his illness, and I was not desired to visit him till the fifth day of the disease. He was immediately subjected to the cold affusion, which was repeated in the evening. This was continued twice a day, the temperature of the water being raised, first to *cool* and then to *tepid*, as his heat diminished. He took saline draughts in the day, with an anodyne at night, and lived chiefly on milk and gruel. There was an abatement of his symptoms immediately, and in four days nothing remained but debility.

Soon after, Mr. Bouker, also a clerk in the house, who had been exposed in the same manner to the contagion, was taken ill. I saw him in thirty-four hours from the first attack. His heat was 105° of Fah^t, his pulse strong and 120 in the minute, and severe pains affected his head and back. We threw over his naked body six gallons of water of the temperature of 60° of Fah^t. in two successive buckets, and replaced him in bed. His pains vanished; his heat and pulse fell to the natural standard; he sunk into a natural sleep, with a breathing moisture over his surface, and awoke in the morning free of every complaint excepting a slight languor. The same symptoms

symptoms occurred in another case similarly situated, and were carried off precisely by the same means. Mrs. Bourne herself escaped. Her son had been effectually washed before she saw him.

The great debility of Mr. Bourne prevented us from taking him out of bed for the purpose of raising the affusion. When the morbid heat has continued, and the strength has admitted it, I have often, in the last four years, used the cold affusion even later in the disease than the period of it, when I first saw him. In the case of a servant of Mrs. Heywood, daughter of my respected friend Dr. Percival, I used it on the 14th day of fever, with the immediate removal of delirium, and every other desired effect.

Few accurate observations have been made of the effects produced on persons under fever, by exposure of their naked bodies to a stream of air. The following may therefore deserve to be recorded;

In the month of May, 1801, I was desired to visit a patient ill of fever in Sparling-street. I found him in the tenth or eleventh day of the disease, delirious and restless; the surface of his
body

body dry, and his heat 104° of Fah^t. The room was close, and I desired the only window in it to be opened. The wind from the north-west blew directly into this window, and the bed being situated between it and the chimney, a pretty brisk stream of air passed over it. The patient had just thrown off a considerable part of his bed-clothes, and was exposed naked to the breeze. I sat by him, with my finger on his pulse, watching the effect. In a little while the pulse fell from 120 to 114 in the minute; he became more tranquil, and soon afterwards he sunk into a quiet sleep, in which he remained when the water for affusion was prepared; of course we did not disturb him. When I left him, I desired the attendants to suffer him to remain in this situation all night, unless he became cold; but to take care to administer the proper nourishment.

Once or twice in the night the attendants placed the bed-clothes on him, but he soon became hot and restless, and they took them off again. While naked he slept tranquilly, and had generally a gentle moisture on his skin. In the morning I found him perfectly collected, and considerably refreshed; his pulse about 100, and his heat 101° . He coughed, however, a little, and we covered him with a sheet, which he now found agreeable to

to his feelings. The cough produced no serious inconvenience, and in a few days the patient recovered under the common treatment.

At the time this experiment commenced, the mercury in my thermometer stood at 65° , and it did not sink lower than 63° during the night. No certain inference can be drawn from the result of a single observation of this kind, but it is not to be doubted, that by a due attention to the heat of the patient, and his sensations of heat, such observations might be safely multiplied, and certain principles at length established respecting the use of cold air as a remedy in fever. In the warmer regions of the earth, where the heat in febrile diseases is probably greater than in our island, and the temperature of the atmosphere higher, a still more free admission of the wind to the naked body may often be useful. The benefit which Dr. Jackson and others have ascribed to gestation or travelling, in certain instances, in the fevers of St. Domingo and America, is probably to be attributed in part, to the mode of conveyance in open vehicles, in which the sick were probably little fatigued by the motion, and were invigorated and cooled by a constant change of air, by showers of rain, and by heavy dews in the

the night *. In the fevers of this country, I have uniformly found fatigue to be injurious, and those who have struggled with them in the early stages, to have a less favourable chance in the issue. It has also happened to me to see, in every instance in which I have had an opportunity of observation, unfavourable effects in fever, from the usual mode of travelling in a post-chaise or close carriage, of some of which I shall presently have occasion to speak. To the invigorating effect of the wind, and the coolness, and refreshment of the dews and the rain, I am also disposed to ascribe the singular recovery, from the worst species of plague, mentioned by Desgenettes, in his *Histoire Médicale de l'Armée d'Orient*, p. 249. "A miner," says he, "attacked by the plague, during the expedition into Syria, escaped naked, during a violent delirium, from the fort of Ca-thieth, and wandered nearly three weeks in the desert. Two buboes which he had upon him at

* These circumstances combined in the decisive examples given by Dr. Jackson, of the benefit of gestation. *Remarks on the Constitution of the Medical Department of the British Army*, p. 296 and 297. I quote Dr. Jackson's narrative of facts with confidence. His inferences, general speculations, and practical maxims, are so much at variance with mine, that I have thought it a hopeless task to attempt to reconcile them.

" the

" the time, suppurated and healed of themselves.
 " He ate when hungry, a small species of sorrel,
 " formerly described." This man perfectly recovered. I have supposed him to have been cooled by the rains as well as dews; for it appears, that during the period of his wandering, the French troops met with heavy rains in the progress of their march, of which in all probability, he felt the influence. Having quoted the work of M. Desgenettes, I cannot forbear to extract the other remarkable recovery, from the worst species of the plague, which he details. " An artillery-
 " man, who had two buboes and an anthrax,
 " (charbon) made his escape from the Lazaretto of
 " Boulak, on the day of his being admitted, and
 " in a violent delirium precipitated himself into
 " the Nile. He was taken up about half an hour
 " afterwards, below Embabeth, by the people of that
 " village, and he afterwards perfectly recovered."
 These extraordinary cures which M. Desgenettes attributes entirely to nature, correspond perfectly with a number of striking facts of the same kind, all pointing out the powerful instinct by which, in the delirium of the plague, as in other burning fevers, the patient is impelled to seek the most easy and obvious modes of relief.

To give these narratives every advantage, they should have contained a statement of the temperature

perature of the atmosphere, of the waters of the Nile, and of the patients exposed to their influence. But the advantages derivable from such observations were unknown in the French army; and what is more a subject of regret and surprise, they seem to have been equally unknown in the English army, as far as the records of the medical practice of our countrymen in Egypt, have come to my knowledge. How fruitless and how perverted are the efforts by which learning and science have in general attempted to combat this fatal disease! The medical departments of both armies seem to have been arranged with the greatest care, but the best remedies for the plague were probably missed by the physicians both of France and England. They were not to be traced in the prevailing systems of medicine, or in the pharmacy of our shops; but it is probable they might have been found, in the refreshment of the breeze, in the dews of night, and in the waters of the Nile.

Having given this general account of the success of the practice recommended in this volume since the last edition, I come now to offer a few observations on certain cases in which its good effects were less apparent. It will be necessary to introduce these with a short narrative.

That

That the year 1801 was particularly unhealthy in Liverpool, has been already mentioned. Three different epidemics prevailed among the inhabitants in the course of that year. The first was the dysentery. This disease made its appearance in the month of July, though it had been prevalent among the French prisoners the preceding winter. It resembled very much the dysentery of the West Indies, as described by Dr. Wright and other respectable authors. Generally speaking it came on with symptoms of diarrhœa. The stools, though frequent and griping, at first consisted of the natural contents of the bowels; but soon became scarce, slimy, and bloody, with increased and almost intolerable griping, fever, thirst, and prostration of strength. In the latter stages, came on sickness of stomach, vomiting, excessive debility, coldness of the surface, and especially of the extremities, delirium, and death.

In Liverpool, as in the other great towns of the kingdom, it is very common for bowel complaints to prevail in the latter end of summer and beginning of autumn; but generally speaking, they are mild in their nature, and easy in their management. In the preceding year these had some instances assumed dysenteric symptoms, but in no case that I saw or heard of with fatal results. The medical practitioners expected at first

first the usual autumnal complaints, and at any rate nothing worse than had occurred in the year preceding, when suddenly they found themselves engaged with a wide spreading and violent epidemic, of great malignity, requiring the most painful attention, and often terminating fatally, in spite of every exertion.

The distress occasioned by the dysentery was speedily aggravated by the appearance of the *Scarlatina Anginosa*, (seldom, indeed, long absent from Liverpool) which spread widely in the month of August, and speedily became general. Soon afterwards the typhus, which is always to be found in the subterraneous dwellings of the poor, burst its accustomed boundaries and extended into the habitations of the opulent, occasioning general alarm. In the months of August, September, October, and the greater part of November, 1801, the degree of sickness in Liverpool, was unexampled in the history of the town. The pressure on the Dispensary was so great as to render it impossible to keep records of the practice, or even of the names of the patients; and the greater part of the private practitioners had little or no remission from anxiety and fatigue. The mortality was considerable. In the months of August, September, and October, there were upwards of fifteen hundred deaths in Liverpool, making an excess

excess of nine hundred above the usual number. In September alone, the deaths amounted to six hundred and ten.

The far greater part of these was produced by the dysentery and scarlet fever. The treatment of the first of these diseases was difficult. On the whole, where the patient was in the first stages, and his strength not much reduced, nor his stomach unsettled, it was a successful practice to clear the first passages, and open the pores of the skin, by Ipecacuanha, or James's Powder, afterwards keeping the bowels regular by small doses of calomel, and allaying the irritation by opium. In other cases saline purgatives, followed by anodyne clysters, proved successful. Castor oil could seldom be retained on the stomach. Calomel was more generally useful, and in some instances it was combined with crystals of tartar, with great apparent advantage. In other cases calcined magnesia, in small doses, (sometimes combined with opium and ipecacuanha, or with opium alone) and followed by successive draughts of lemonade, answered every purpose. In a few cases I made a trial of the remedy of my friend, Dr. Wright—sea salt, dissolved in vinegar or in lemon-juice. Used in the early stages of the disease, this remedy seemed to answer the character he has given of it; but in the latter stages it failed, as indeed the communication I had from

Vol. II C him

him led me to expect. I regret that I did not employ it more generally.

These and other methods, varied according to circumstances, and pursued with the utmost determination, though generally successful, in several instances failed. Out of a hundred and ten cases of the disease, which I saw in private practice, all of them in a situation to procure the necessary assistance and attendance, ten died. These were chiefly persons advanced in life, or of infirm constitutions; but two were young and vigorous, and seemed to have every chance in their favour. In this disease, the heat, after the appearance of the dysenteric symptoms, rose to 102° and 103° , the tongue became furred, the skin dry, and the pulse from 100 to 120 in the minute. In the progress of the disease, the heat sunk below the natural standard, as has already been mentioned, and the pulse became feeble and less frequent. I did not try the cold affusion or the application of cold in any form, having learnt by experience that it does not succeed in fever with affections of the bowels. I tried, however, the tepid affusion in a few cases, and though with abatement of heat, with no lasting benefit. The patients complained of the fatigue and pain of moving, and of the chilling effects of the remedy, which was therefore abandoned.

In

In the *Scarlatina* I used the affusion, both cold and tepid, with the happiest effects, as shall be more particularly mentioned immediately.

I also used the affusion universally in the typhus fever of that season, but with less striking advantage than on former occasions.

The typhus at that time differed somewhat from its usual character among us. It came on in a less obvious manner. For some days the patients felt dull headache, languor, and debility, with slight chills at intervals, and uneasy nights. The tongue was lightly furred. In many instances the disease was mistaken for a common catarrhal affection, and its real nature not discovered till the extraordinary progress of debility excited uneasiness and alarm. In its regular course, the disease was protracted beyond the usual period of typhus, extending to eighteen or twenty days, or even more, and sometimes leaving behind it, for a week or two, a state of mind bordering on fatuity. The skin was at no period after the first or second day, particularly dry or constricted; and in twenty-three cases, which I attended closely, and examined with care, I did not find in a single one, the heat exceed 102° , at any period of the disease. In general the heat was from 98° to 101° , and greatest about the

fifth or sixth day. Of the twenty-three patients which I have mentioned, I lost two. Both of these had undergone extraordinary fatigue. The one, a lady of thirty years of age, had been for some days ill in Wales, (where she was on a visit) before the nature of her complaint was discovered. When it was made known to her, she insisted on returning to Liverpool; actually set off in a post chaise immediately; and performed a journey of thirty-eight miles, six of which were by water, in a single day. She arrived oppressed with fatigue, and almost lost in stupor. Her heat did not justify the cold affusion, but the tepid affusion was employed with obvious though transient relief; and in spite of every support from wine, bark, and nutrition, she sunk under the disease, on the thirteenth day. The other case was that of a young man, in his seventeenth year, of the most steady character, and the most amiable disposition of mind. Being intrusted with business of consequence, he concealed his indisposition in the first days of the fever, and continued to perform duties of considerable fatigue and exertion, in the course of which he was repeatedly affected with sickness and faintings. When I saw him first, he was scarcely able to walk across the floor, it being the fifth day of his fever, though only the second of his confinement. The affusion, at first cool, and afterwards
 tepid,

tepid, afforded momentary relief, but made no serious impression on the disease. We had recourse to wine, bark, æther, and musk, and endeavoured to sustain his strength by the most nourishing diet, but he sunk into insensibility and expired on the seventeenth day of the fever.

In the treatment of the other twenty-one cases, there was little difficulty. The affusion, seldom lower than *cool*, but generally *tepid*, was employed in all of them, sometimes once, and more frequently twice a day. It was always followed by refreshment, and immediate relief, but had less effect in cutting short the disease than usual. I did not even succeed in stopping it, in cases in which this remedy was applied on the second or third day; but this might be owing to my seldom having recourse to the perfectly cold affusion, from the unusual deficiency of heat in this epidemic. Perhaps a practice somewhat bolder might have been more successful.

During the prevalence of the influenza in spring, (1803;) a few cases in my practice assumed the form of typhus, all of which recovered easily, with one exception. This was the case of a young married lady, whose loss, I, in common with all who knew her, deeply lament. Residing at some distance from Liverpool, she had

there been seized with the influenza in its usual form and had been nearly confined to bed for three days, when she received an account of the dangerous illness of a near relation in Liverpool, to whom she had from early infancy been strongly attached. No consideration could prevent her from setting off to see this relation immediately. She rose out of bed, threw herself into a post-chaise, and performed a journey of thirty-eight miles without stopping. She arrived a few hours only before her relation expired, and these hours she spent by the bed of the dying person. The sensibility of her natural constitution had been heightened by the influenza, and was excited to an extraordinary degree by the circumstances of this melancholy scene. She kept her feelings however under command, but sleep deserted her, and when at last it returned it was accompanied by stupor, and interrupted by uneasy sensations and frightful dreams. For several days however, the pulse was very little more frequent than natural, the skin was soft, and the morbid heat inconsiderable. She was affected with the symptoms of influenza, in rather a severe degree, and with great agitation of spirits, but there was no appearance of pressing danger. The tepid affusion had a soothing influence, but was not as usual effectual in producing tranquillity and sleep; and the cold affusion was not employed. In this state

state we ventured to remove her from the scene which served to remind her of the subject of her grief, to a house about a mile distant; she bore the journey well, and expressed satisfaction in the change. But on the afternoon of that day the symptoms suddenly assumed a serious form. The pulse became rapid, the breathing oppressed, the nervous startings alarming, and delirium impended. That fatal symptom, a morbid sensibility of the surface and of all the organs of sense, augmented rapidly, and delirium supervened. She lived however four days, and in the last twenty-four hours of her life, her consciousness and recollection returned. She foresaw her fate; prepared for it with the most perfect resignation; and on the morning of the 14th day from the first attack of the influenza, breathed her last. The tepid affusion was used in this case a few times only, before the disease assumed the typhus form. The debility, agitation, and diminished heat, did not allow me to propose it afterwards. The usual medicines were prescribed, and the usual means of supporting strength were employed—in vain!

In the latter end of last July, a fever occurred in the case of a gentleman of great worth and respectability, a good deal resembling the above, but still more nearly resembling the fever described in chap. viii. vol. i. This patient, previous to the

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attack, had been much affected in his spirits. His stomach had for a considerable time been disordered, and he had been afflicted with severe and almost constant headaches. He was in an enfeebled state when the fever came on. The symptoms were very exactly those described in p. 46, 47, and 48, vol. i.; excepting that the heat of the patient never exceeded 102° , and continued at this height for a short time only. But there was that morbid sensibility all over the surface, and in the senses of hearing, sight, and taste, which I have already pointed out as a symptom of so fatal a nature. Delirium came on the tenth day; at first it occurred at particular moments only, but afterwards it became constant. The patient in this case survived till the eighteenth day. The tepid affusion was used often in the early stages, and generally with immediate, but transient relief. The symptoms did not seem to justify the use of the cold affusion, and it was not resorted to. Wine, opium, bark, musk, æther, and sinapisms, were employed to keep up his strength, but with no apparent advantage. Petechiæ and vibices covered the surface in the latter stages of the disease. After death, a great discharge of blood took place from the nostrils, and the body went rapidly into putrefaction.

I have thus related all the instances which have occurred to me since the last edition of this volume,

volume, (a period of five years of extensive and attentive observation) in which the affusion of water on the surface of the body, cold or tepid, proved either less beneficial in its effects in fever than I had formerly represented it, or entirely unsuccessful. I would add, if any such had occurred, the instances in which this remedy had appeared to be injurious. But experience has suggested to me no instance of this kind, and extensive as my employment of the affusion has been, I have never heard that it has suggested even to the fears or prejudices of others, a single occasion of imputing injury to the remedy. If I were to detail in the same manner the evidence in its favour, which has occurred to me during the same period of time, it would occupy many volumes. In the months of September and October, 1803, twenty cases of fever occurred in my private practice, in all of which the affusion of water was employed with success, having either cut short the disease, or conducted the patient in safety through it.* It is true in all these cases, other remedies were used, for it would

* Of these cases, seven were arrested in their course, the rest passed safely through the disease. In only two families out of thirteen, did the fever spread after the use of the affusion, and in both instances within ten days of the affusion being first employed.

be unjustifiable for the sake of experiment to neglect any means of safety; yet these remedies were of the most simple kind,—saline draughts, small doses of laudanum, and mineral-acid drinks, with milk, gruel, and occasionally wine. In the use of all these remedies, and particularly of opium and wine, the strictest attention was paid to the heat of the patients, without which he who undertakes the treatment of fever seems to me to *walk in darkness*.

Hitherto I have rigidly adhered to the rule of not using the affusion of water excepting where the heat is greater than natural; and to this I ascribe the general success and the uniform safety that has attended the use of this powerful remedy,

The second rule which I laid down, of not employing the cold affusion even in cases where the heat is preternaturally great, if the patient has a sense of coldness upon him, I have also adhered to in the few cases of this kind, which have occurred to me. In these cases, as in hydrophobia, there is not merely an extraordinary sensibility of the surface to cold, but to every other impression, accompanied by a corresponding sensibility in the senses of hearing, sight, and taste. Such cases have very generally been attended by spasmodic affections of the voluntary muscles, restlessness, and delirium, and have uniformly

formly terminated fatally ; opium, bark, camphor, wine, æther, and musk, proving wholly useless, if not injurious. The case mentioned in chap. viii. and some other circumstances, led me to believe that the cold affusion would prove equally inefficacious, and to lay it down as a rule, that it ought not to be employed, even though the heat should indicate its use, if this sensibility of the surface to impressions of cold were present. I confess that my experience was not sufficiently ample to justify me in laying this down absolutely, and in a species of fever so generally fatal under all the old methods of treatment, it may be thought right that this new and powerful remedy should have a more ample trial before it be set aside. This reasoning will be supported by the following important and very clearly detailed case, furnished me by Mr. Dalrymple of Norwich, which I shall give at length in his own words. Certain parts of it I have marked in italics.

“ On the 31st of January, 1802, after a few days of slight indisposition, James Money, aged 16 years, of a healthy habit of body and serious turn of mind, was seized with a long continued and very violent shivering fit, which was quickly succeeded by a greatly encreased state of his temperature. He complained grievously when I first saw him of intense lancinating pains in the
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head;

head; of sickness and oppression at the pit of the stomach; of great uneasiness in the region of the loins, accompanied by extreme prostration of strength, and a distressing sense of soreness over the whole surface of his body. His tongue was covered with a thin cream-coloured crust; his pulse was small and quick; his heat was now natural; his eyes were dull and suffused, and an air of deep despondency overspread and saddened his whole countenance.

“Some days previous to the appearance of these symptoms he had been exposed to the contagion of typhus fever, by occasional attendance on the sick bed of his father, and had suffered considerable agitation and distress of mind in consequence of a severe domestic misfortune. His elder brother who had been more uniformly about the person of his parent, was seized, nearly at the same time, with milder symptoms of a similar kind; and although he obstinately declined all medical assistance, passed safely through a mitigated typhus of the ordinary form. James however, at the desire of his master, became my patient, and was ordered to take an emetic dose of tartarized antimony immediately, and in the evening, when he should have been sometime in bed, a gentle anodyne draught. I visited him

him on the following morning, and found him in a very restless and perturbed state. The emetic had operated moderately, and somewhat relieved the oppression at the præcordia, but he had passed a sleepless, unquiet night. He complained heavily of his head and loins ; his respiration was embarrassed and frequent ; his tongue dry and brown ; thirst great ; urine high coloured and scarce ; pulse 102, and small ; his heat was still moderate and natural, but the tenderness of his surface was become so great, that on passing my hand under the bed clothes, in order to ascertain the state of his pulse, he screamed dismally, from a dread of the pain he expected to suffer from my touch.

“ In this state he continued, with very little variation of feelings and appearances, until the evening of the 5th of February ; during which interval he had been ordered to take frequently small doses of the compound powder of ipecacuanha, alternated with doses of decoction of the yellow bark, and sulphuric acid ; beer, wine, and opium had also been given in moderate quantities, and a blistering plaster, had been applied, with some little advantage, to the nape of the neck.

“ On the morning of the 6th of February, I perceived in him a material alteration for the worse ;

worse; his animal heat, which had hitherto continued uniformly moderate, was then greatly encreased, the quick-silver of Fahrenheit's thermometer applied at the axilla, rising to 104 degrees. His sense of hearing was become wonderfully acute, insomuch that he was considerably incommoded by noises which were either obscurely, or not at all perceived by others: his sight was also greatly quickened. He was fretful and refractory; talked sometimes calmly, at other times very wildly; was extremely restless in his bed, answered, sometimes prematurely, constantly with eagerness, to such questions concerning him as were asked of the nurse. He fluctuated greatly in his spirits in the course of a few minutes, being now elated with joy, at his self-assurances of recovery, now depressed with despair, from conviction that he should die. In addition to the other means that had hitherto been used, attention was now directed to be given to the state of his heat, and his body was ordered to be spunged frequently with a mixture of cold vinegar and water: but this process he greatly disliked and constantly opposed; *for although he was much distressed by a sense of burning heat, he was so apprehensive of the effects of cold air upon his skin, that he was constantly collecting the bed clothes together,*

together, and wrapping them close round him. Cold acidulous drinks, however, he eagerly called for and largely drank. At eight o'clock of the same evening I repeated my visit to him, and entering his room I found him sitting up in his bed talking and singing loudly and deliriously. He answered rationally however to some questions that were put to him; complained heavily of his head, and of the action of the lighted candle upon his eyes. *His pulse was 120; his heat increased to 108°, his skin felt parched and dry; the crust on his tongue was of a dark brown hue; and from the commencement of his illness, on the morning of the 31st of January, to the evening of the 6th of February, his nights and his days had been equally sleepless.* Under these circumstances I determined to make a trial of the cold affusion, a remedy I considered as still in reserve, and which I had hitherto been deterred from employing by the moderate state of his animal heat. As soon, therefore, as the necessary conveniences were prepared, he was taken out of his bed, conveyed into an adjoining room, and before he was aware of what was intended against him, a pailful of cold water was hastily poured over his naked body.

“The shock was unexpected and severe. He
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started from his seat as the water was falling upon him, and endeavoured to make his escape, but being restrained, he wrung his hands, wept bitterly, and earnestly entreated he might be permitted to return to his room. Wrapped in a warm blanket he was conveyed back to his bed. *In a few minutes afterwards his pulse was examined, and found to beat 100 strokes in a minute; his heat, which an accident prevented me from accurately measuring, was most sensibly diminished; his mind became calm and clear; he expressed a feeling of regret for the trouble he occasioned to those about him; drank a glass of warm wine and water, and in about half an hour he sunk into a deep sleep in which he continued nearly eight hours.*

“ When I saw him the following morning, his skin was moist and cool; his pulse 96 and firm; his thirst gone; the pains in his head and loins removed; his countenance was cheerful; his intellect collected and composed, and he appeared only like one suffering from extreme debility. But in the course of the day, his heat again increased; in the evening his pulse was quickened to 108 strokes within the minute; his tongue was dry and thirsty; he became restless and anxious, and complained considerably of his head and loins. His body
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was, therefore, ordered to be sponged copiously and frequently with a mixture of cold vinegar and water, and although he once expressed very violent dislike to that remedy, he now submitted himself to its application without reluctance, and derived from it effects at once agreeable and useful. He slept soundly and perspired gently during the ensuing night; awoke in the morning, refreshed and free from fever; the dark brown crust had left the edges, and was quitting the middle of his tongue; his pulse beat 90 strokes, and firmly; the pains in his head and loins were removed; he ate his food with appetite and relish, and with a few slight checks and interruptions, eventually recovered his ordinary state of health."

In a letter from Mr. Dalrymple, dated 1st February, 1803, he expresses himself as follows.

"I was originally induced to trouble you with the paper, (that which is printed above) because I conceived that the very striking proof which it contains of the powerful influence of *cold affusion*, upon a disease marked by circumstances so unusual and so alarming, could not fail to be welcomed by the author of the *Medical Reports*.

“ Within the space of six years immediately preceding the period when it occurred, I had seen two instances of fever, in which the concomitant symptoms were nearly similar to those that appeared in James Money’s case. In each of them, the excitability of certain of the organs of sense was greatly encreased; and in each, that singular libration, the alternate elevation and depression of mind, which is certainly one of the most curious and may, probably, be a necessary quality in the character of this modification of fever, existed in a more or less obvious degree. In both instances the patients were healthy, robust young men, of dispositions somewhat grave, and inclined to melancholy; and like Money, they had, previously to the commencement of their illness, suffered considerably from mental anxiety and bodily fatigue. Their disease was also distinctly traceable to contagion. It was treated as a typhus; that is to say, an emetic was prescribed in the first instance, and in the subsequent periods, wine and bark, with the sulphuric acid and opium; and blistering plasters were prescribed, according to the state of the living powers at the time. In both cases, the event was unfortunate, and therefore in Money’s case, I was led to form and to pronounce unfavourable prognostics. The general powers of cold affusion were not indeed
unknown

unknown to me, for I had previously employed it with uniform success, in several instances of common typhus ; and in the summer of the year 1801, when Scarlatina was ravaging every quarter of this city, I had seen great advantage derived from its early application, in a few instances of that formidable malady. But as it was impossible for me not to trace, in the symptoms of my third patient, a very close resemblance to those described in the 8th chapter of your book, I confess I had little or no reliance on its efficacy. I considered the use of it, therefore, in the light of a mere experiment, rendered justifiable by the probable failure of all other remedies, and by the safety with which, I knew from experience, it might be made.

“ The result was eminently successful, and I considered the communication of the fact to you as an act of simple justice, due to a writer, whose works, on a very difficult and important subject of medical enquiry, I had read with great pleasure and advantage.

“ My subsequent experience has furnished me with no case that can authorize a particular detail. The only diseases in which I have at present advised cold affusion are Scarlatina and the common putrid fever : but the result of my trials

of it has been such as will certainly cause me to enforce its exhibition whenever opportunities present: my practice, however, does not carry me extensively among that class of society which is most exposed to the influence of typhus contagion."

I very earnestly hope, that future experience may establish the safety of using the cold affusion, in situations similar to that described by Mr. Dalrymple; and that one of the restrictions which I was induced to lay down on the use of this remedy, may be modified or entirely removed. It will still remain doubtful, whether cold, or even tepid affusion, can be applied with advantage, where fever is accompanied by dysentery, or inordinate discharges of the bowels of any kind; or whether it can be applied with safety, where it is attended by local inflammation. But the establishment of Mr. Dalrymple's practice would make it unnecessary to attend to the sensations of the patient; and the rules for the use of this remedy would be rendered more simple and precise.

I have been the less anxious to extend the use of the cold affusion to the Phlegmasiæ and Hæmorrhagiæ, because a remedy has lately presented itself, that greatly enlarges our power over the numerous diseases which are arranged under these orders:

orders: I mean the *Digitalis Purpurea*. This medicine may almost be said to be possessed of a charm for allaying inordinate action of the heart and arteries, and in this point of view, as well as for its efficacy in some kinds of dropsy—particularly hydrothorax, its introduction into medicine is one of the greatest benefits our science has received in modern times. The extraordinary power of the *Digitalis* in the *Hæmorrhagiæ*, and particularly in *Hæmoptysis*, is pretty generally known, and if it were necessary I could confirm it by some striking examples: * its use in the *Phlegmasiæ*,

* I lament that in confirmed phthisis pulmonalis, the hopes entertained of it have not been confirmed, though it is of essential benefit in the predisponent state, and even in the incipient stages of the disease. In families, where this fatal disease is hereditary, the use of this remedy, as a prophylactic, will, I have no doubt, save many a life that would otherwise have been cut short.

Dr. Rush, and some other of the physicians of Philadelphia, have lately revived the practice of employing mercury in confirmed Phthisis Pulmonalis. This practice had entirely failed in the hands of my predecessor, Dr. Dobson, and in those of Dr. Duncan, and the late Dr. C. Webster; and I should not have had recourse to it anew, had it not occurred to me that it was reasonable to try it in combination with the *Digitalis*, though each might have separately failed. Accordingly, about twenty months ago, I put five cases of confirmed

masiæ, is, so far as I know, in a great measure new. Digitalis does not, indeed, supersede the use of the lancet in these diseases, but it diminishes the extent to which it is required; and it may be used with safety and success, in cases where the lancet can no longer be employed. Under the precautions pointed out by Dr. Withering, without the strictest attention to which no practitioner should prescribe this singular and powerful medicine, I have employed the Digitalis to a very considerable extent in inflammations of the brain, of the heart, and of the lungs; and have succeeded with it in situations where I should otherwise have despaired. I have also

firmed Phthisis Pulmonalis on a course of mercury, within a short space of time, having first bridled the circulation in each, by means of the Digitalis. I pushed the mercury so far as to affect the mouth in all these cases. They all, however, terminated fatally, though I really think they were all prolonged. In one of them, the effects appeared so considerable, that at one time I was led to entertain a slight degree of hope. The brother of this last patient was threatened with the same disease, and put on the same course the beginning of last winter, with some appearance of advantage. Being, however, in the greatest apprehension of the effects of the winter, I advised a voyage to Lisbon, which he undertook the beginning of 1804, and returned the ensuing summer, free of complaint. In a disease so fatal under all the established modes of treatment, this practice deserves, I think, a farther trial.

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found it an excellent remedy in inflammatory rheumatism, one of the most tedious and intractable of diseases. At some future opportunity I may, perhaps, offer my more mature experience on this subject to the public. In the mean time I congratulate our profession on having obtained a direct and unquestionable *sedative*; a term, which after the example of Dr. Ferriar, I apply with confidence to the Digitalis, in spite of the recent systems which proscribe the word; systems, which if they were otherwise stable, the extraordinary, and in some respects opposite powers of this potent medicine would confound and overthrow. The prognostic which Dr. Ferriar gave to the world in 1799 *, respecting the use of the Digitalis in inflammatory fevers, and which my experience has confirmed, I have the pleasure to learn by a recent communication from himself, has been amply justified by his own subsequent experience.

* Essay on the medical properties of Digitalis purpurea, by J. Ferriar, M. D. Manchester, 1799.

CHAP. II.

The Subject continued—Application of the cold and tepid affusion to Scarlatina—to confluent Small-pox—to Meazles—to Influenza—General Remarks.

THOUGH I have hitherto abstained from the use of the cold affusion in the Phlegmasiæ in general, considering the presence of topical inflammation as in some measure precluding its use, yet I regard the subject as inviting enquiry in the case of Erysipelatous affections.

In several of the Exanthemata it may be employed with striking advantage. No one will doubt that it is applicable to the eruptive fever of the small-pox, a disease happily becoming rare among us; but it is not equally known, that it may be used with the most singular benefit in the eruptive fever of Scarlatina, whose ravages are becoming every day more extensive and more familiar all over Europe, and for which no Jenner has yet arisen to propose a sovereign antidote.

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In chap. ix. vol. 1. some account is given of the first trials of the cold affusion in Scarlatina, now proceed to give the farther results of the application of affusion both cold and tepid in that disease. I shall premise a few general remarks on its history and character.

Some traces of the existence of Scarlatina may be found in the writings of very remote times, but the very different symptoms which it assumed in different epidemics, made its history for a long time imperfect and obscure. When Dr. Fothergill improved the treatment of this disease in the year 1748, its nature was very much unknown. Dr. Cullen, who, as he himself informs us, saw the scarlatina five or six times epidemic in Scotland, has described it under two distinct names, Scarlatina, and Cynanche Maligna, considering these different species to be distinct diseases. Dr. Withering, in the first edition of his work on scarlet fever and ulcerated sore throat, published 1778, adopted this division, which in his edition of 1793, he has abandoned; and physicians are now pretty generally agreed, that both proceed from the same contagion, and are therefore different degrees of the same disease. The varieties of the Scarlatina, are in fact, not greater than the varieties of the small-pox, to which they bear a very strict analogy.

Another question has arisen among physicians, of considerable importance—whether Scarlatina, like the small-pox and measles, occurs to the same person once only, or whether like the plague and other contagious fevers, it may return indefinitely. Having been taught the last opinion, I adhered to it for many years, as on the whole the safest; but never having seen a single fact in confirmation of it, though I have known the Scarlatina to appear in many instances for the second and third time in the same family, I now consider the same individual to be liable to Scarlatina once only. I have indeed heard of one or two instances to the contrary; and Dr. Heberden is of opinion, that such have occurred; but it must be admitted on all hands that they are rare, so rare indeed, as scarcely to require to be taken into account in our practice.

It is true, persons attending on patients under Scarlatina, are sometimes affected with sore throat who have themselves formerly gone through the disease. But this is not attended by the scarlet efflorescence, or the general affection of the system by which the regular disease is ushered in. The affection of the throat is primary and topical, though I have known it so painful as to bring the system at large into sympathy. It seems to be produced by inhaling the breath of the patient,

patient, and is probably analogous to those partial eruptions of small-pox which sometimes appear in mothers or nurses who have had that disease, from the contact of infants who are under it.

That the experience on which I give these opinions may be justly estimated, I may mention, that for the last twenty years the Scarlatina has never been a whole year together absent from Liverpool, and that besides the single cases which are often occurring, there is scarcely a year that passes in some part of which it is not more or less epidemic. The following observations, as well as those preceding, must therefore be considered as the result of personal observation.

There are cases of this disease so slight, that the affection of the throat produces no inconvenience, and may be wholly overlooked. In this form it appeared to Sydenham and De Gorter. I have known single cases of this kind, but never knew this to be the general character of the disease when epidemic, as must sometimes have been the case, if we may trust the descriptions of the physicians just named. No one dies of this species of Scarlatina. Rest, quiet, and diluents are alone required. It may be compared to the mildest species of small-pox.

On the other hand, there is a species of Scarlatina to which the name of Purpurata ought rather

to be given, for the efflorescence is of a purple, not of a scarlet hue; in which, though the throat be deeply and extensively ulcerated, the pain and difficulty of swallowing are comparatively small, for the passage is kept open, and the sensibility of the part destroyed, by the progress of gangrene. In such cases, extreme feebleness and rapidity of the pulse, and great force of the breath, appear even in the commencement of the disease. The heat does not rise much above the standard of health. Great debility, oppression, head-ach, pain in the back, vomiting, and sometimes purging, accompany its rapid progress: the patient sinks into the low delirium, and expires on the second, third, or fourth day. This disease is to be treated by large quantities of bark and wine, and the other remedies employed in gangrene. The cold affusion is scarcely applicable to it, and the tepid affusion makes little impression upon it. In my experience indeed, all remedies have been equally unsuccessful. It outstrips in rapidity, and it equals in fatality, the purple confluent small-pox, to which it may be compared. Happily it occurs rarely. I have not seen it more than five or six times in upwards of twenty years practice.*

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* The last case of this kind occurred in May, 1802, in a family in Thomas-street, Liverpool, which I attended, with
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These are the two extreme forms under which Scarlatina appears. But the form which it has assumed in Liverpool, with few exceptions, so far as my experience or enquiries extend, is intermediate between these extremes; sometimes leaning more to the one, and sometimes to the other; this observation applying not to particular cases only, but generally to the epidemics of different seasons. This is the Scarlatina Cynanchica of Dr. Cullen, the Scarlatina Anginosa of Sauvage; by which last name, as being most generally adopted, I shall speak of it.

After some previous lassitude or weariness, of uncertain duration, the Scarlatina Anginosa comes on with the usual symptoms of pyrexia—shivering, pain in the back and head, nausea, and frequently vomiting; in proportion to the violence of these symptoms, and to the rapidity of their progress, is the danger of the disease. In an hour or two

Dr. Minshull. One of the children had the scarlatina in a very mild form: another had it very severely—with this the father except: he was in his office of business on Friday, and was dead on Monday morning. His symptoms were those described—purple eruption, &c. He had the tepid affusion approaching to cold, with little benefit. The different species of this disease which existed at one time in the same family, all springing from the contagion, were alone decisive of the question respecting the identity of Scarlatina Anginosa, and Cynanche Maligna.

morbid

morbid heat comes on and speedily mounts up far beyond the temperature of health, this accession of heat being generally attended by a great sensibility and bright-red flushing over the whole surface of the body, with some stiffness of the neck, hoarseness of the voice, and rawness of the throat. *

If the thermometer be applied to the surface of the body after the sensation of heat has become steady, the mercury will be found to rise to 105° , and 106° , even in mild cases, and in the more violent cases, to 108° , 109° , and 110° . I have known it to rise as high as 112° , the greatest heat I ever observed in the human body. It is on the first appearance of this high temperature that it is necessary to act with vigour. On our conduct at this critical season the patient's life often depends.

The plan that I follow, if called in at this early period, is to strip the patient, and dash

* The great heat in this disease was noticed by some of the early writers on the subject, particularly by Sennertus; but it has been very much overlooked by the moderns, and was never at any time before ascertained by the thermometer, so far as my enquiries extend. I consider it as the most important feature of the disease. Scarlatina Anginosa is probably the hottest of all the diseases to which we are subject; it is certainly the hottest of all the febrile diseases of this climate, a fact which I have ascertained by actual admeasurement of them all.

four or five gallons of the coldest water to be procured, over his naked body. This produces its usual cooling effects ; but these are less permanent than in typhus. In one or two hours afterwards the heat is often found, on examination, as great as before. The affusion is therefore repeated again and again, as the obstinacy of the heat may indicate. It is sometimes necessary to use it ten or twelve times in twenty four hours. At the end of this time, but commonly earlier, the force of the fever is broken, and a few tepid affusions, at longer intervals, are sufficient to subdue it entirely. During this time, cold water and lemonade should be used as drinks, and the bowels opened, if necessary, by calomel. In a few cases I have thought it advisable to assist the affusion by the diaphoretic power of a solution of tartarized antimony. If left to myself I use no other means.

Considerable languor and debility, with a disposition to rest and sleep, follow this bold arrest of the fever. I have seen these appearances such as to excite some uneasiness lest coma were coming on, or the powers of life sinking. But I never saw any real ground of alarm, and it is sufficient to keep up, if necessary, the heat at the surface of the body, and particularly of the extremities, by integuments, leaving the patient to that profound repose in which nature

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ture delights after violent agitations. On the third day very generally; or sometimes the fourth, the patient is convalescent. If the throat be examined, there will be found some fulness and redness, and perhaps some white specks on the tonsils, but nothing that can be called ulceration. There are, of course, none of the secondary symptoms to which ulceration gives rise. No ichorous discharge corroding the neighbouring parts, descending down the æsophagus, and producing diarrhœa;—attacking the epiglottis and larynx, and occasioning croup; or entering the absorbents and producing buboes in the parotids, or other glands of the neck. Neither does inflammation or irritation pass along the eustachian tube to derange the functions of the ear, or ascend to the brain itself, and disturb the sources of life. In no instance did I ever see delirium come on after the use of the cold affusion.

The peculiar dropsical affection indicated by the swelling in the hands and feet *does* frequently occur, and sometimes there is a slight cough. These pass away of themselves, or if necessary may be removed by the digitalis and crystals of tartar.

In cases where, from the timidity of parent or the apprehensions of those with whom we are called

called to consult, this decisive practice cannot be fully adopted, the tepid affusion may be had in course to with very considerable, but inferior effect. It will not arrest the disease unless very slight, but it will moderate its violence, by moderating the heat, and in the end producing sensible perspiration. It was observed to me by my friend, Dr. Clark, of Newcastle, whose great sagacity and extensive experience are entitled to every respect, that in some instances, where he had used the tepid affusion with immediate but transient diminution of the heat of fever, he had afterwards had recourse to immersion for ten or fifteen minutes in the tepid bath, with more permanent effect: he found from this immersion a strong, sensible perspiration, which almost every day on the disease, has laid down as its favourite crisis, generally considering this as the means by which the noxious cause is expelled from the system, and not perceiving, that it is the process by which nature in all cases throws off superabundant heat.

Where I do not see the Scarlatina Anginosa till the third or fourth day, or even later, if the violent heat continue to be great, I use the cold affusion; if less considerable, the affusion cold or lukewarm. It is still an excellent remedy; diminishing heat and irritation, and producing quiet sleep; &c. &c.

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but though it lessens, it cannot arrest the disease. If the ulcers of the throat are foul, and the breath fœtid, an infusion of Cayenne pepper, stronger or weaker, according to the sensibility of the parts, makes an excellent gargle : half a grain of pepper to an ounce of water is a proper strength to begin with. This was recommended to me by the late Mr. Macbeth of Demerary, and it deserves the praises he gave it. It is detergent and antiseptic in a high degree ; it may even be given internally with advantage, in those cases which sink into debility and putrescence, and where bark and wine are required.

These last remedies, experience has taught me to use much more rarely than formerly, and always with a reference to the heat of the patient and dryness or moisture of his skin.

During the fifteen years which preceded 179 I had much experience in the established remedies in Scarlatina Anginosa. To the use of blood-letting and purgatives—a fatal practice—the use of bark had been substituted, and certainly with great comparative advantage, though with very imperfect success on the whole. The effective administration of bark in this disease is generally indeed a matter of great difficulty. The pain and irritation attending deglutition of

render the patient most averse to swallowing it in the form of powder or decoction, even where he may be influenced by rational motives, and produces an invincible repugnance to it in the great majority of patients, whose tender age does not admit of their conduct on such occasions being regulated by their understandings. In such cases a perplexing situation arises; either we must abandon the remedy, or administer it by force, under the uncertainty of its producing benefit, and the certainty of the injury arising from the mode of its being administered. To escape from this painful dilemma, bark has been given in the form of elysters, by which there is no doubt that its effects on the system, though in an inferior degree, may be induced; and it has been applied to the surface quilted in linen or cotton, on the supposition that the absorbents of the skin might take up some portion even of the dry powder in this form; a notion which seems to me utterly foundationless, while the bark jacket, as it is called, is evidently calculated to do mischief, from the heat and irritation it must certainly occasion.

Far be it from me to reject the authority of the numerous and respectable names by which bark is recommended in this disease. The effects of this remedy are not immediate or obvious, and are therefore not very easily ascertained; but I

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must be allowed to say, after long and attentive observation, that I am doubtful of its producing benefit, and that I am inclined to ascribe the superior success which followed its introduction, rather to the abandoning the former fatal system, than to the direct influence of that which was adopted.*

The effects of wine are more obvious; this remedy has often been grossly misapplied. In the hot stages of Scarlatina, with a dry skin, it is highly injurious; but in the after stages, when the heat is abated and the skin open, especially if the strength be sinking, and the ulcerations in the throat assuming a putrescent form, it is a remedy of the first importance. Since I began the treatment by affusion, I have however had very little occasion to use it either in this or in the typhus fever.

Previous to the adoption of this treatment, I had, on a full comparison, convinced myself of the general superiority of the mode of practice of Dr. Withering—the early use of emetics—to any other,

* The extract of the letter from Dr. Fothergill to Dr. Withering, (see Dr. Withering's account of scarlet fever, p. 10), clearly shews that Dr. Fothergill himself, became doubtful of the beneficial effects of bark in this disease.

and I have also to say, that I entirely agree with that excellent physician in the reprobation of blisters to the neck, from which I never in a single instance could perceive benefit, and from which I have suspected very detrimental effects.

In the years 1798, 1799, and 1800, several cases of Scarlatina occurred, in which I employed cold and tepid affusion, according to the degree of heat and the stages of the disease, with very general success. I was fully prepared therefore for the treatment of the wide-spreading and fatal epidemic which broke out the latter end of the summer of 1801. My family was at that time in the country, where it was seldom in my power to visit them. The scarlet fever had appeared among the children in their vicinity, and carried off several. My two youngest children who had not had the disease, both boys, one five, and the other three years of age, had been in company with some of these children at play, and had been exposed to the contagion. I gave directions that they should be watched narrowly, and that I should have intimation of the first appearance of complaint. On the morning of the fifteenth of Aug. a message was sent me, that the eldest of the two had been restless and uneasy in the night, with fever, chills, and pain in his head and back. I saw him in seven hours from the first of these chills;

he was then becoming hot, and had vomited up his tea: his face and neck were beginning to flush, and it was evident that he was attacked by Scarlatina. His younger brother had constantly slept in the same bed with him; though then walking about, he was evidently spiritless and languid, and there was little doubt that he also had caught the disease. In a little while the eldest boy became very hot, and the youngest sick and restless. He followed his brother, step by step, at the distance of about seven hours. The heat of the eldest soon raised the mercury of the thermometer to 106° , 107° , and 108° , and in both, the symptoms prognosticated a violent disease. I had lost a girl of four years of age in Scarlatina a few years before, though her first symptoms were far less violent; she perished in consequence of the ulcerations extending to the epiglottis and larynx, and producing the symptoms of genuine croup. I shut myself up with these boys; and with plenty of pump water and a pocket thermometer, prepared, not without anxiety, to combat this formidable disease. It would be tedious and useless to go into details. As soon as the sensation of heat was steady in my eldest boy, I stripped him naked, and poured four gallons of water over him, of the temperature of 64° . The usual good effects immediately appeared, but at the end of two hours he was as hot as

+ *The weakness of affection. v. vol. 1. p. ever*

ever—the remedy was again applied, and repeated as the return of heat indicated. By the time the eldest was ready for his third affusion, the youngest was ready for his first. The heat rose in the eldest to 109° . in the youngest to 108° and the pulse in each was upwards of 150. In thirty-two hours the first had the affusion fourteen times; eight times cold, twice cool, and four times tepid. Twelve affusions sufficed in the case of the youngest, of which seven were cold. The fever was in both completely subdued. On the morning of the third day they were both evidently safe; and on the morning of the fourth, though the pulse was still a little more frequent than natural, they were both convalescent. In this state they inclined to sleep and rest. The scarf-skin peeled off them both and each had a slight degree of swelling in the hands, but none of the other secondary symptoms.

I might multiply these details, for the epidemic of that season would afford me ample scope; but this would be tedious and unnecessary—One other narrative, illustrative of the effects of the affusion in different stages of the disease, shall therefore suffice.

In the same month (August, 1801) the Scarlatina appeared in a back court out of Peter-street, which contained eight small houses, and forty-

eight inhabitants, twenty-six of which were children.* The two first that were affected, died; several others were taken ill, and consternation seized the rest of this little community. It happened that Mr. Barr, foreman in Mr. M'Creery's printing-office, lived in this court, and that two of his children were affected by the disease. He applied for my assistance, and I undertook of course to give it the others also. I found nine children in different stages of Scarlatina, but all admitting of the use of affusion, cold or tepid. I had not the slightest difficulty in persuading their parents to use it; they all followed implicitly the example of Mr. Barr. The mode of applying it was simple enough. The weather was warm, and the patients were brought out into the middle of the court, naked, where the water from the neighbouring pump was dashed over them. As the heat declined, the water was made tepid. Not only was the affusion employed for the sick, but once a day for the children in health also. It

* For a description of these Courts, see p. 347, vol. i. The particular court mentioned above, communicates with the street by a covered passage, 26 feet long, and only 2 feet 10 inches wide, and this is the only exit or entrance. The court itself is 54 feet long, and 7 feet 10 inches broad, and contains eight small houses, four on each side, the doors of which front each other, and open into the court.

might serve as a preventive, and at any rate it promoted cleanliness, which was enforced as essential—Ventilation was also promoted to the best of our power,

After this system commenced, four or five others were seized with Scarlatina, who all had the disease in the most favourable way. Those in whom it was advanced, when I first saw them, went through the secondary symptoms—one of them with severity; but the whole recovered. No medicines were used, except a beverage of water acidulated with muriatic acid, an infusion of Cayenne pepper as a gargle, and calomel, where a cathartic was required—Milk, broth, and gruel formed the nourishment.

By these simple, but powerful means, were death and disease banished out of these sequestered families, and health restored in fourteen or fifteen days. It is not a little curious, that ten children, all susceptible of Scarlatina, so far as was known escaped it entirely.

A circumstance occurred during my attendance which deserves to be mentioned. One of the children, supposed to be taken ill of this disease, was uncommonly oppressed in the first stages, and the heat much lower than in any other case. It varied

varied from 99° to 102°. No doubt however being entertained of the complaint, this child was subjected to the cold affusion during the eruptive fever, in the same manner as the others. But as the disease proceeded, it turned out to be the purple confluent small pox, and the patient died, as is usual in such cases, on the eleventh day of the eruption.*

A case nearly similar occurred in the present year. When the 24th regiment of foot was quartered here about eight months ago, I was desired by the assistant surgeon to visit its hospital.—Among other patients there was a young soldier just brought in; he had been affected by fever about twenty-four hours before, and was particularly oppressed in his head and stomach, with violent pain in his back—he had shivered and vomited severely. On examining his heat, it was much less than usually accompanies such symptoms in fever,—there was a hoarseness in his voice and his face was a little swelled, and darkly flushed. I advised the tepid affusion, approaching to cool, which was several times repeated. The disease turned out to be the small pox, of the purple

* Mr. Barr, who is quoted in the course of the above narrative, is the person whose hands are employed in setting it for the press. (first edition).

confluent kind, and terminated fatally, notwithstanding every support from food or medicine. It appears then that there are cases of small-pox, as well as of Scarlatina, which do not receive the usual advantage from the cold or tepid affusion, and that in the one disease as well as the other, these are cases which show little morbid heat in the eruptive stage, and in which great malignity and putrescence appear from the first.

To return to Scarlatina—this disease continued prevalent during the autumn of 1801, and throughout the succeeding winter and spring; and though less frequent since, it may be said to have been constantly present in Liverpool, in a greater or less degree, up to the present time. In all the cases which I have seen during this period, amounting to upwards of a hundred and fifty, I have uniformly followed the practice which I have just described, and with a degree of success so nearly invariable, that I cannot contemplate it without emotions of surprise, as well as of satisfaction. In the course of this time, I have had occasion to combat the Scarlatina twice in public schools, and in both instances was completely successful, not merely in the recovery of my patients, but in stopping the progress of the disease. The use of this remedy undoubtedly strengthens the confidence in the means of prevention, recommended in the
writings

writings of Dr. Haygarth, Dr. Clark, and Dr. Blackburn, and now generally adopted by the scientific part of our profession. I have received various interesting communications respecting the success of this practice in Scarlatina, from several of my medical friends, particularly Dr. Rutter, Mr. Dale, and Mr. Eaton, to which I would readily give a place here if my limits would admit. I cannot however refuse myself the pleasure of publishing the following communication from Dr. Gregory, Professor of the practice of Medicine in the University of Edinburgh, which reaches me at the moment that this sheet was going to the press. The weight of such an authority will be duly appreciated by the world; and Dr. Gregory's evidence shall be given in his own full, clear, and forcible language.

“ Edinburgh, 9th Nov. 1803.

“ Dear Sir,

“ It is to inform you of part of my family distress, and at the same time to thank you for a piece of very valuable practical instruction, which I received from you two years ago, indirectly, and at second hand, that I write to you at present.

“ You will remember that, about two years ago,

ago, you informed Dr. Wright* of the success which had attended your practice of cold affusion, in fifty, out of fifty-two, cases of Scarlatina. Dr. Wright shewed me your letter, and as your observations appeared to me very interesting, I transcribed into one of my note books that part of your letter, *verbatim*, and have read it the two last winters in college, when treating of the Cynanche Maligna; telling my pupils that I had no personal experience of the practice, but that I thought your testimony in its favour, and the analogy of the good effects of the very cold practice in continued fever, and in natural small-pox, so strong, that I was resolved to try your practice of the affusion of cold water in Scarlatina, the first good opportunity; meaning the first recent, violent case, with great heat, and frequent strong pulse, that should come under my care.

“It happened that I saw none but slight and very favourable cases of the disease till last August, when I was called to a girl of thirteen, who had been ill of the Scarlatina, with a very bad sore throat, for near a week. She seemed to me very likely to die, and in fact did die, in a few days.

* Dr. Wright, with whose important narrative this work commences.

“I mentioned

“ I mentioned in consultation the cold practice by affusion, but could not urge it, for two reasons ; first, because the circumstances of her disease seemed to me very unfavourable for it ; the hot state of it was past, and the symptoms of debility, putrefaction, delirium, stupor, and even some watery bladders on the skin (which I have very seldom seen) were come on. I could not expect to save her by your practice, and I could easily foresee, that an unsuccessful use of it would have brought a reproach upon the practice, and would have prevented it from being followed in other cases, in which it might probably have done good. In the second place the girl's own situation was so peculiar, * * * * * that I was very unwilling, for moral and prudential reasons, to try any new and rough practice on her ; for if she had died under it, which most probably would have happened, it might have been said, that she was chosen as the subject of a severe and dangerous experiment, because she was a helpless, unprotected orphan. So she had her chance by the best of all other means, that I knew of, for her relief, particularly bark, wine, acids, fruit, the cool regimen, great cleanliness, and ventilation ; and, at one time, opiates and astringents, on account of colliquative diarrhœa ; but all to no purpose.

“ The next patient in Scarlatina that I had
occasion

occasion to see, was a nephew of my own, the eldest son of my deceased brother, a stout boy of fourteen, of the melancholic temperament. Whether he brought the contagion of it with him from England, whence he came in the beginning of October, or whether he got the contagion in my brother-in-law's house, near Edinburgh, which I suspect was the case, as the children of that family had had the disease last winter, though in a very mild form, I know not; but soon after he arrived in Scotland, and in that house, he was attacked with Scarlatina. The case proved a pretty smart one, with ulcers in the throat, copious scarlet efflorescence, hot skin, and pulse 120; but as I saw no symptoms of danger in it, I did not urge the cold affusion, but told him, that if he grew worse he should be soured in the most complete manner. He recovered perfectly well, and soon, without it; and after much purification of himself, his clothes, and his baggage, was received into this house three weeks ago.

"This day se'ennight (1st Nov.) my eldest son, a boy of six years of age, of a sanguine temperament, and very irritable constitution, was taken ill about noon, when out at his walk. Your friend, Benjamin Bell, saw him before I did, for having called on me to speak about a patient, he enquired for my wife, and found the child with her,

her, lying on a settee in her dressing room. She anxiously begged of him to examine the child, and see whether he had got the scarlet fever, which, from the circumstances already mentioned, she suspected might be the case. Mr. Bell had no difficulty in pronouncing at once that the disease was the scarlet fever; for even by that time (within two hours from the invasion) the eruption of red prominent points was very copious, and evident, so as to give a general roughness and redness to the skin of his breast. In that state I found him soon after Mr. Bell left him; his pulse frequent, and small, his skin but just beginning to grow hot, and his hands rather cold. He complained of general oppression, uneasiness, and head-ach. The velum pendulum, uvula, and tonsils, were of a dark purple red; but there were no ulcers or aphthæ on them, that I could see. Before six at night, he had much more severe head-ach and oppression, with nausea, vomiting, and diarrhœa; his pulse 140, and very strong, his skin very hot, and of a bright red, with a considerable roughness all over it.

“Conceiving this to be a case of the most imminent danger, and, in every respect, proper for your cold practice, I lost no time in getting him taken out of bed, stript, and set erect in a tub: in which situation, I poured a gardener’s watering-

watering-pan full of cold water all over him, from head to foot. This relieved him a good deal; and, as you may believe, cooled him very effectually: but before ten at night, the symptoms had recurred with more violence than ever. His pulse was 160, and very strong, his face turgid, his eyes growing red, his skin very red from head to foot, and very hot. I did not take time to measure the heat of his skin by the thermometer; but I think it must have been at least 104° of Fah°. I immediately repeated the cold affusion as before, and with the same good effect. He was immediately cooled and refreshed; and half an hour after, I found his pulse 120, and the heat of his skin very moderate. He had a pretty good night; but before eight next morning was become very hot again; on which account the affusion was repeated in the same manner. He said he did not like my way of bathing, and would rather be bathed in his own tub; and as I thought, from the state of the symptoms, that there was occasion for it, he was bathed in his own tub, and fairly dived under water, over head and ears, between seven and twelve at noon, and again at six in the evening of the second day.

"These five good sousings, in twenty-four hours, seemed to me to have done the business completely. The eruption was not repelled, but

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the progress of the local affection in the throat seemed to be stopped. I could never see any ulcerations in it, though I looked carefully for them, in consequence of having observed much fœtor of his breath on the second evening. The force of the fever, as to heat and frequency, and strength of pulse, seemed to be quite broken after the fifth ducking; to such a degree even, that I began to have some fears that the *vis vitæ* might fail, and had actually provided wine to be at hand, to give him from time to time, in case of need, and saw him several times during the night between the second and third day of his disease. But I found no occasion to give him any of it; nor has he had a grain of medicine, or even a drop of wine during his illness; so that you must allow it has been a very fair experiment of the cold affusion, as a remedy for the scarlet fever. Indeed ever since the morning of the third day, (forty-three hours from the invasion of the fever) I have considered him as a convalescent, though he had occasionally, for three days afterwards, some transient quickness of pulse and heat of skin. His throat was quite well the morning of the fourth day; the same day he had some slight swelling of his hands; but not the twentieth part of what I have often seen after a severe attack of Scarlatina. He has had for some days, a kind of crick in his neck, or pain extending from his right

right ear towards the shoulder, which makes him carry his head awry, inclined to that side ; and I can feel some of the lymphatic glands enlarged ; but they are not bigger than large pin heads. He is losing his cuticle by desquamation, as was to be expected ; he has been washed with tepid water and soap for three or four nights, to promote that desquamation, and relieve the itching of his skin. He has not yet recovered his flesh, strength, or colour ; but seems in a fair way to do it soon, for he has a keen appetite and digestion, and, for three or four days has been able to play as usual in his nursery ; this being only the ninth day of his disease.

“ You may be sure, from this detail, that I am perfectly well pleased with your practice in this disease : and that I shall gladly follow it in any proper cases of the same kind that may come under my care, if I am allowed to do so. I can now propose and urge the practice with a good grace, and some confidence, after having tried it with success on a child of my own. It is more than possible that I shall soon have occasion to try it on two other sons of my own ; one of them past four, the other of them almost two years of age, and both of them of the same sanguine temperament of their eldest brother. Both of these younger children were exposed to the contagion

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from

from their cousin, my nephew, as much as my eldest son was, and both of them were with *him* when he sickened, and for two hours after, so that I could have no hopes of completely preserving them from the contagion; I therefore allowed them to be fully exposed to it, by sleeping in the same room with their elder brother, and playing with him whenever he was able to play. The two younger children are also the subjects of another experiment: both of them have had the cow-pox, which my eldest son had not. Him I had inoculated with the small-pox, many months before I ever heard of the cow-pox.

“ Thus far I had written, (Wednesday, Nov. 9th) and meant to have said that the experiment going on in my nursery, might help to ascertain the point, which I understand has been a matter of conjecture and speculation, whether the cow-pox preserve those who have it from the scarlet fever, as it seems to do from the small-pox, and as it is said, from the plague. If my two younger children escaped it, when so completely exposed to the contagion, it would give some countenance to that favourable opinion with respect to the cow-pox; if either of them took the scarlet fever, it would effectually put an end to that opinion; for *major est vis instantiæ negativæ, in omni axiomatico vero constituendo*; but the experiment

ment in that respect is already finished ; and that point settled unfavourably for the cow-pox*. Yesterday afternoon, my youngest son sickened, and had the usual symptoms of oppression, sickness, vomiting, diarrhœa, frequent crying, frequent pulse, copious red eruption on the skin in less than three hours after he began to be uneasy, flushing of the face, and increase of heat ; but neither the frequency of pulse, heat of skin, or flushing of the face, have been nearly so great as they were in his brother. I have not been able to see the inside of his throat, but as his voice is somewhat affected, and hoarse, I presume he has a slight degree of Cynanche ; but he swallows both liquids and solids without difficulty, so that I have no great fears from the state of his throat. His eyes have been less red, but more watery, than his brother's were, and he has had a very copious salivation, which his brother had not.

“ Though he was never so hot as his brother, he was washed in a tub with cold water, about seven o'clock, and again about eleven, last night, and twice to-day already with tepid water, as he was still less hot than yesterday, and his feet even cool. He has been relieved by the washing all the

* The universal adoption of the cow-pox in Liverpool, and the prevalence of Scarlatina, had decided this question long ago there.

four times, and has always gone to sleep after it for a longer or a shorter time. At present I see no symptom of peculiar danger about him.

“ My other son (the four-year-old gentleman) has not yet sickened; but I expect him to be taken ill to-night or to-morrow: for last night he had very little sleep, and frequently through the night, and to-day, he has complained of being tired. *Lassitudines spontaniæ morbos denunciant*, is one of the oldest maxims that I know of in physic. Whenever he sickens, I mean to treat him as I have done his brothers.

“ This long and minute detail will perhaps not be altogether uninteresting to you, as the author of that practice, which I have followed: but that I may follow it the more confidently in other cases, and be enabled to answer some of the many questions that will be put to me about it, I beg you will inform me, if you have made any other interesting observations respecting it; and particularly if you have observed any bad effects, either immediately or remotely from it. How long have you observed the febrile state to continue after its force was broken by the cold affusion? Have you observed more or less of the anasarca swellings of the extremities after this, than after the common practice? Have you observed after it any symptoms of ascites, or of hydrothorax,

hydræthorax, or any affection of the head, such as coma, delirium, or convulsions, or any parotids. Has the convalescence after this treatment been quicker or slower than usual?

“ I trust I need make no apology for giving you all this trouble.

Yours most truly,

J. GREGORY.”

To the questions of Dr. Gregory I replied in course of post. As however the answers given may be clearly inferred from what I have already said of the effects of the affusion applied early in preventing the secondary symptoms, it is unnecessary to insert my letter at length. The result of the whole was, that my experience afforded no grounds for any of the apprehensions implied in the very judicious inquiries which he had made. I concluded with a request to be informed of the progress of the disease in his family. This was complied with in the following letter,

Edinburgh, 16th Nov. 1803.

“ Dear Sir,

“ I thank you for your kind attention in giving me such a full and speedy answer to my long

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letter

letter. It is most completely satisfactory to me in every respect ; and I am sure it will give you satisfaction to know that my children have ALL done very well, under your practice for the Scarlatina. The eldest and youngest, though they have not yet fully recovered their strength, are recovering it very fast, are in excellent spirits, and on the whole so well, that they have been out of doors repeatedly, both in the carriage and on foot ; though the weather has been very cold, even with frost and snow, for some days. Both of them have still some swelling of the lymphatic glands on both sides of the neck, extending from the ear almost down to the shoulder. This I think the more remarkable, as I could see no ulceration in the throat, and no particular cutaneous affection on the hairy scalp of either of them. The swelled glands are no longer painful, and are already growing smaller and softer : so, I presume, they will discuss completely without any difficulty.

“ The extraordinary salivation which the youngest child had, was soon and completely explained. It appeared that the scarlet fever had brought on, or at least accelerated, a fit of teething, and that two new teeth were just cutting the gum : they are now quite through, and of course he is perfectly at ease : but, for four or five days after the Scarlatina was subdued, he seemed to suffer

suffer more from the irritation of teething, than he had done from that often severe and dangerous fever.

“ The four-year-old gentleman whom you enquire after, sickened on Monday morning, 14th November, or sometime in the night between the 13th and 14th. He had been complaining for five days, much and frequently, both day and night, of being tired; at day-break on Monday he complained much of headach, and, upon looking at his skin, he was found to be from head to foot, as red as a boiled lobster. His pulse for two days, was nearly as quick as his elder brother's had been, (from 130 to 150) but it was not nearly so strong, nor was his skin so hot as his elder brother's had been. The eruption still remains very copious (Thursday afternoon, the fourth day of his disease.) There is very little redness, no dark purple colour, and no ulcers in his throat, which I have seen, all, and repeatedly every day. He had no vomiting or diarrhoea, as both the others had; but his body continued quite open as in his perfect health. Supposing the headach, which in him was very severe, to proceed from the stomach and bowels, being in some measure loaded, I gave him on the afternoon of the second day, three grains of calomel, which did not operate as a
 purge,

purge, but soon, and very effectually as an emetic; and seemed to remove his headach; but as this returned on the morning of the third day, after being suspended for about sixteen hours, two good leeches were applied to his temples, by which he lost a good deal of blood, with great and immediate relief to the headach; which has not yet returned, and I presume, never will return.

“The cure in this boy, as in the other two, was trusted to the washing; but as his pulse was not very strong, nor his heat very great, the water was not perfectly cold, though very gently tepid; so little warm as to make him shiver. It seems to have succeeded as well with him as with either of his brothers: the heat of his skin, which is still red, is little more than natural; his pulse is come down to 108, he has eat his dinner to-day with a good appetite, and about an hour ago I saw him walking, or rather staggering about his room; but I presume he would soon tire of that and go to bed again, as he is still weak. I conceive him to be a fair convalescent.

“I have had much pleasure in observing repeatedly in the youngest child (the two-year-old gentleman) the great and immediate good effect of the cold or tepid washing, not only in lesser

g the frequency of pulse and heat of skin, t in relieving the febrile oppression and uneasiness. The little patient who just before was lying very much, unable to hold up its head, incapable of being pleased or amused with anything, nay almost incapable of looking at anything, immediately after being washed, (I mean two or three minutes) would begin to look up, and take notice of the people near him, then amuse himself with his playthings, then get upon his legs and run about upon the floor, and at last fall quietly to sleep.

“ From what you mention in your letter, I am sure such observations must have been quite familiar to you.

Yours most truly,

J. GREGORY.”

To comment on cases related with such circumstantiality and precision, would be to weaken their effect, and with this single observation, that I suspect the heat in the first of Dr. Gregory's cases to have been several degrees higher than he supposed, I leave this interesting narrative to the selections of the reader.

Before

Before I conclude the subject of Scarlatina, I must *again* enforce the superior advantage of using the affusion early in this disease; and the propriety of ascertaining that the skin is dry, and the heat of the patient greater than natural, in all cases, especially in such as are advanced, and where, of course, the strength is considerably impaired,

It has come to my knowledge, that in two cases of Scarlatina, of the most malignant nature, the patients have been taken out of bed, under the low delirium, with the skin cool and moist, and the pulse scarcely perceptible. In this state supported by the attendants, several gallons of perfectly cold water were madly poured over them, on the supposed authority of this work. I need scarcely add, that the effects were almost immediately fatal.

It has happened in several instances, that the cold, cool, and tepid affusion, have been used in the early stages of the Cynanche Tonsillaris, or common inflammatory sore throat. Though I should not have used it in such cases by design, I never found injury to arise from it. On the contrary, it was uniformly followed by a mild disease.

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I should have been still less inclined to have described it intentionally in the measles, on account of the disposition to pulmonary affection which attends that disease. It has happened to me, however, to have directed it four different times by mistake, in the eruptive stage of measles, and in like manner, the disease that followed was singularly mild in every instance.

When the Influenza was prevalent last spring, I did not employ the affusion on account of the rough and pulmonary symptoms which attended that disease. I contented myself with diaphoretics, blisters, and diluents. My patients recovered under this treatment, though in some instances, slowly, and with difficulty; especially in cases where there had been a previous asthmatic or consumptive tendency. Though constantly exposed to this disease, I escaped for nearly two months; but at length was seized by it severely at the beginning of May. Being of a phthisical constitution I was not without anxiety as to the debility it might induce, or the pulmonary affection it might leave behind; and reflecting on the remedies I had employed, it did not appear to me that any of them had materially shortened the disease. I determined therefore to try the cold affusion, approaching to cool, and subjected myself to the shower bath every three or four hours.

hours regularly, at the temperature of 85° . The effects were in a high degree grateful, soothing, and invigorating. I suffered nothing from pulmonary affection, either during or after the disease. The debility went off very soon, and I recovered more speedily than any of my patients. The great peculiarity of the Influenza seemed to be the speedy debility which it induced, and the morbid sensibility of the nervous system by which it was accompanied. The heat in this disease was not great. It varied from 99° to 101° , and 102° , which is pretty nearly the temperature of measles. Though I attended to the subject, I am at a loss to decide whether the Influenza was contagious or not. If contagious, it spreads by laws peculiar to itself.

It will be proper to say a few words of the affusion of warm water on the surface of the body that is, water of the temperature of the blood and upwards. This produces a very considerable sensation, of a highly grateful nature, but is followed by a great degree of chilliness, and sometimes by pulmonary affections in persons disposed to them; in others by catarrhal affections and the other symptoms which indicate what is meant by the common expression of having *caught cold*. I have used it chiefly in maniacal affections, and sometimes in them with soothing effects

ects. That it produces a powerful influence on the system of sensation, the following case will shew.

H. D. a young man in an apothecary's shop in this town, intending to take an ounce of tincture of rhubarb, swallowed by mistake an ounce of laudanum. He immediately perceived his error, and took as quickly as he could, but not till two minutes had elapsed, three grains of tartarized antimony, attempting at the same time to bring on vomiting, by irritating the internal fauces. Finding his efforts unsuccessful, he took almost immediately six grains more, and a sudden terror seizing him, from perceiving the effects of the laudanum, he left the shop, and ran as fast as possible to my house, (a distance of about three hundred yards) for further assistance. I was sitting in my study, when I heard a furious ring at the outer-door, which was instantly opened, and the young man rushed in upon me, with marks of the greatest agitation. Before he had time to speak, vomiting came upon him, and learning the circumstances of the case from the master of the shop, who followed close after, I encouraged the vomiting by warm water, and incessant irritation of the fauces. Experience had taught me that there is no safety without keeping up vomiting for a considerable time, and it was continued for half

an hour or upwards, at the end of which time the stomach became unirritable, and debility and stupor increased upon him. He however contrived to walk home with considerable difficulty, supported all the way.

When laid upon a settee, his eyes appeared suffused and heavy; his pulse was 95, and rather feeble; and drowsiness, notwithstanding constant external impression, was fast gaining ground. In this state, I directed several gallons of warm water to be poured on his naked body, which had the singular effect of removing entirely the drowsiness for about ten minutes; but it returned again, and he could scarcely be kept awake by constant shaking. This agitation however brought back the vomiting, and he threw up the vegetable acid which had been directed for him. The warm affusion was repeated a second time, with the same effects as at first. The tongue soon after looked white, the skin grew hot; and the pulse rose to 105. The warm affusion was repeated a third and last time; immediately after which, a very cold fit took place, with great tremor and faintness. He was put into a warm bed, and allowed to sleep, but the disposition to it was gone; about nine hours after the accident, he was able to take ságó, and fell asleep. In this state

state he continued through the succeeding night, and awoke in the morning languid, but refreshed and free of all complaint. I give this case chiefly from the notes of the gentleman affected.

The use of warm water was in the first instance accidental in this case—I had ordered the tepid affusion; but observing the water to be very warm, (probably 106° or 108°) as it flowed over him, and that a great effect was produced, it was continued of nearly the same temperature.

CHAP. III.

*Communications to the Author respecting the
Use of the Cold and Tepid Affusion in different
parts of Great Britain.*

BEFORE I presented to the world an account of the use of water externally and internally, as a remedy in fever, I had witnessed its effects for a period of ten years, and for five years of that time in hospital practice, under circumstances most favourable to accurate observation. By this means in announcing the remedy, I was enabled to lay down certain rules for its administration, which if not perfect, were however safe; and I had it in my power to exhibit its effects, on so large and so varied a scale of observation, as to render it improbable that the results could be substantially erroneous, though they might not be precisely correct. My publication was favourably received at home, and the second edition translated in

the French and German languages. The progress of the practice it recommends has been as rapid as I could have hoped, or perhaps wished; for it was not my desire that it should be embraced with the vehemence of enthusiasm, but that it should be received after slow and cautious investigation; and it was impossible for me not to deprecate its being employed at all by those practitioners whose character of mind, and habits of life, rendered them impatient at the close, and sometimes painful attention necessary to the administration of such powerful remedies. In the space of six years this practice has however made considerable progress through the island; there are few parts of it in which it is unknown, and there are some in which it may be considered as fairly established.

In London, I have reason to believe, that it is now making some progress, * and its success under the direction of Dr. Dimsdale, in the hospital for fever, denominated the House of Recovery, is so striking, that though the account of

* See the paper of Mr. Blegborough, in vol. viii. p. 158, of the Medical and Physical Journal, and that of Mr. Pearson, in the same volume, p. 357.

it is already given to the world, I think it proper to insert it here.

Extract from an Account of Cases of TYPHUS FEVER, in which the Affusion of cold Water has been applied in the London House of Recovery. By W. P. DIMSDALE, M. D.

CASE I.

“ James Johnson, aged eight years, caught the infection from his parents, who died of fever. He was removed on the 19th May, 1802, into the House of Recovery. On the 23d of May (the twelfth day of the disease) the symptoms were as follow:—pulse extremely frequent tongue covered with a dark fur, and very dry skin dry. A thermometer placed under the tongue arose to 104° : constant and violent delirium. The usual medical treatment not being attended with success, recourse was had to the affusion of cold water. He was taken out of bed, stripped, and a pitcher of cold water was poured suddenly over him: after being wiped, he was replaced in bed. He slept an hour; the skin felt more relaxed; no perspiration however followed.

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May 24th, pulse 120; skin dry; heat 100°; delirium continues; no sleep in the night. The affusion was repeated with a pail of cold water. He again slept quietly, was evidently more collected when he awoke; and soon afterwards a profuse perspiration came on, which continued through the night. On May 27th (the fourth day after the cold affusion had been first used) he was entirely free from fever.

CASE II.

“ Thomas Knight, aged twelve years, was admitted June 16th, on the fifth or sixth day of typhus. In the afternoon, pulse 116; skin dry, with numerous petechiæ; heat 104°; eyes suffused; violent pain of the head. The cold affusion, with a pail of water, was directed. The pain of the head subsided, he slept quietly, and copious perspiration followed. From this time the symptoms were favourable. On the 22d he was free from the disease, on the fourth day after was removed from the house.

CASE III.

" John Harrogan, aged twenty-six years,
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came into the house on July 8th, the fifth day of the disease: pulse 120: tongue furred and dry; skin hot and partially moist; delirious at intervals; pain of the head and back. July 9th, violent delirium came on in the night, two nurses were unable to keep him in bed. The matron of the house sent for me at five o'clock this morning; he was then extremely outrageous; pulse 136; skin hot and parched. He was placed by force under the shower-bath, and two pails of cold water were poured instantly over him. The transition from a state of extreme fury, to perfect calmness, was truly surprising. Without an effort of resistance on his part, he was replaced in bed: profuse perspiration succeeded. In three days he had no symptom of fever remaining.

CASE IV.

“ Alfred Sweeting, aged four years, was removed into the House, 13th July: he caught the infection from his mother, who died in a small and dirty apartment. On July 15th fourth day of the disease, pulse very frequent skin dry, heat 102° ; tongue slightly furred countenance expressive of much uneasiness. The shower

shower-bath was used: he appeared immediately to be much relieved; general moisture of the skin followed. On the 16th he was free from fever. This patient took only the saline mixture, and afterwards small doses of the diluted nitrous acid.

CASE V.

“ Henry Hancock, aged twenty-eight years, was on the 10th of August, the fifth day of typhus, removed into the house. Pulse 120; tongue furred, slightly moist; skin very dry; heat 105°; severe pain of the head. The shower-bath was directed. The pain of the head was removed instantly; perspiration succeeded. The symptoms continued favourable to the 14th, when he had no complaint remaining except weakness.

CASE VI.

“ George Johnson, aged fifteen years, came in on the 13th of August. On the 14th (fifth day of typhus), pulse 124, heat 98°, slight partial moisture of the skin: the tongue furred, and

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much

much general uneasiness. August 15th, he has been very delirious in the night, and extremely restless: complains of violent pain of the head; pulse very frequent, tongue furred, rather dry; skin dry, numerous petechiæ over the body; heat 103° . The shower-bath was immediately used. The pain of the head was instantly removed, but no general perspiration followed. In the evening the head-ach and the other febrile symptoms returned with nearly the same severity as before. The cold affusion was again used, and he felt immediate relief. Copious perspiration very soon succeeded, which continued through the night. He was free from complaint on the 17th, the third day after the first use of the cold affusion.

CASE VII.

“ John Beard, a boy aged eleven years, was admitted on the 21st of August, in the third day of fever, with the usual symptoms: pulse frequent; much thirst; pains of the head and back; the skin rather moist. 22d, skin dry, heat 103° , pulse 116, tongue furred; pain of the head continues. The cold affusion was directed immediately, and applied again in the evening. He passed

passed the night easily, the skin was partially moist: he had some refreshing sleep. August 23d, the skin is now dry; heat 104° ; complains as before of much pain and general uneasiness. He again used the shower-bath. In the evening, the skin being dry, and the heat 102° ; was repeated: profuse perspiration came on in the night. 24th, skin very moist; heat 98° ; pulse 100; tongue slightly furred; says he feels much better. In the evening, during a short absence of the nurse, feeling a slight return of heat and uneasiness, he poured a pitcher of cold water which was in the room over himself into the bed. The nurse returning immediately, she removed him to a dry bed: he slept quietly through the night, the skin moist, and awoke in the morning quite free from fever. The only medicines ordered in this case, were the saline mixture, and small doses of Colombo.

CASE VIII.

“ Abraham Johnson, aged twenty years; was admitted on the 6th of September, with the usual symptoms, in the fifth day of fever. Sept. 7th, skin dry; heat 100° . The shower-bath was used, which produced considerable relief. On the

the 9th the heat was again 100° ; the skin dry. The cold affusion was repeated. He was free from fever on the 12th.

CASE IX.

“ Mary Johnson, aged eleven years; removed into the house August 13th, in a late period of fever. She relapsed August 22d. On the 23d, pulse 132: tongue covered with a dark fur, rather dry; skin dry, heat 103° ; pain of head and back. Copious perspiration succeeded the cold affusion, and in two days she was entirely free from fever.

CASE X.

“ Robert Holmes, aged twenty-six years; admitted September 7th, with fever of uncertain date. Pulse 100, tongue slightly furred; pain all over the body. In the evening the skin became very dry; heat 100° . A slight delirium with which he was affected, subsided immediately on the use of the shower-bath. He was free from fever on the 10th, but extremely feeble. By the use of a nourishing diet, and small doses of
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the bark and wine, he gradually regained his former strength.

CASE XI.

“ John Dutchfield, aged twenty-one years; admitted on the 25th of September, with the usual symptoms of fever. On the 28th (ninth day of the disease) skin dry, heat 100° ; used the shower bath; the heat diminished, the skin became moist. On the 2d of October he was free from fever.

CASE XII.

“ Mary Simmons, aged forty-two years, was admitted November 18th into the house, with the usual symptoms of fever; the date uncertain. On the 20th, pain of the head exceedingly violent; skin dry; heat $99\frac{1}{2}^{\circ}$. The head-ach ceased immediately after the cold affusion, the skin became rather moist. On the 23d, the heat again rose to $99\frac{1}{2}^{\circ}$; the skin dry; copious perspiration followed a repetition of the affusion. She was free from fever on the 25th.

OBSERVATIONS.

OBSERVATIONS.

“ It appears unnecessary to relate the other cases in which the cold affusion had been used. In all, the good effects of it have been strikingly manifest, and in no instance has the disease terminated fatally after the use of this remedy. In the early stages of typhus the affusion, with very little assistance from medicine, appears to cut short the progress of the disease. In the more advanced periods, when the strength of the patient is sufficient to admit the application of this remedy, it moderates the violence of the symptoms, and contributes materially towards a favourable termination. When the strength is greatly exhausted, it may probably be wholly inadmissible. The patients almost invariably expressed great satisfaction, after the agitation immediately following the affusion had subsided. The violent pain of the head, so distressing in fever, is almost constantly and immediately removed, and generally, quiet sleep succeeds, with moisture of the skin.

“ Case the 7th furnishes a strong illustration of these remarks. The boy always after the first affusion, went to the bath with perfect readiness, and even solicited its repetition. The
almost

almost immediate discovery of the affusion which he had himself practised, prevented any injurious consequences; and it is evident from the report of the following day, that the slight exacerbation of fever which came on in the evening, was completely removed by this application. The feelings of the patient in this instance immediately prompted him to have recourse to the remedy, from which he had before experienced so much relief*.

“ Spring water has been used hitherto without any addition. A shower-bath is placed in the House of Recovery for the purpose of applying the remedy. It is obvious that the affusion is by this means rendered more complete than by any other mode of application; it is also neater, and more commodious. Ablution of the body, by sponging with cold or tepid water and vinegar, has been frequently employed with advantage: it is however less effectual than the affusion.

* “ The reader who is desirous of information as to the use of the *cold affusion* (or of the *tepid bath*) in cases of *scarlet fever*, is referred to Dr. Currie’s Medical Reports, p. 60, 61, and 62; and to some other parts of that excellent work.” Dr. Dimsdale.—See in this vol. chap. ii.

J. C.

“ I shall

"I shall feel peculiar gratification if this short account, by confirming the facts stated in the elegant and truly valuable publication of Dr. CURRIE, should tend to accelerate the general introduction of a remedy so important in the treatment of fever; being fully convinced from the uniform success which has attended the practice, that it may be used with perfect safety in this disease, "when (to use Dr. Currie's words) there is no sense of chilliness present, "when the heat of the surface is steadily above "what is natural, and when there is no general "or profuse perspiration."

5th Jan. 1803.

This account was obligingly communicated to me by Dr. Dimsdale on its first publication. Four months after, I wrote to enquire the result of his farther experience, and received the most satisfactory reply. It had, during that time, been used very extensively, and except in two instances, with invariable success. The two patients who died, he observes, were both admitted on the seventh day of fever; no remedy had been employed previously to their admission, and the cases were so extremely violent and irregular, that they appeared to be nearly hopeless. The cold affusion relieved the more violent symptoms tem-

porarily, but did not prevent the fatal termination of the fever. He adds, that from a comparison of these with other cases which have occurred in the House of Recovery, it may be fairly presumed that the remedy would have been effectual in an earlier stage of the disease.

At Edinburgh, the metropolis of the northern division of the island, ablution of the surface in fever has been practised for several years with great advantage by Dr. Gregory, and perhaps by others. During the winter 1802-3, the cold and tepid affusion were employed in sixty-four cases of fever in the clinical ward of the Edinburgh Infirmary, by my friend Dr. James Home, with 'extraordinary, I believe I may say, invariable success. In most of these cases the disease was too far advanced, on admission, to allow of its being cut short; but in such cases as presented themselves in the early stages, and particularly in some of the nurses who caught the infection from the patients, its power of arresting the fever was evident and striking. Dr. Reeve observes, "the number of patients labouring under fever admitted last winter into the clinical ward, was unusually great. In the treatment of these patients, which was extremely judicious in every respect, the most striking and marked advantages were

were derived from the affusion of cold and tepid water. To the manifest influence of these powerful agents, our clinical professor, Dr. Home, jun. attributed with great modesty, and apparently with great justice, the very favourable termination of so many and such dangerous cases. As far as observation during three years will enable me to judge, the continued fevers of Edinburgh are generally attended with catarrhal symptoms. In most of the patients in the clinical ward last winter, these symptoms were strongly marked. In some cases the cough was so violent as to require blisters, and even bloodletting. The danger was still farther increased in these instances by violent head-ach, great prostration of strength, delirium, and petechiæ. The skin in most cases was very hot and dry. The greatest degree of heat ascertained by the thermometer was 106° in the case of a young girl. The most important point, as you have justly remarked in your Medical Reports, which required to be established by experience alone, was the safety of employing the affusion in cases of fever combined with symptoms of local inflammation: Now the frequent trials made at Edinburgh afforded most satisfactory results on this subject. Not one of the patients who had symptoms of catarrh, or inflammation of the lungs, suffered the least inconvenience from the cold or tepid affusion; none of their symptoms were ag-

gravated

gravated or increased by it, but on the contrary most of them were relieved. The tepid affusion was generally employed when the catarrhal symptoms were strongly marked. The effects of this application did not exactly correspond with those stated in your 10th chap. In some instances it did not diminish the heat of the skin, or the frequency of the pulse, even in cases where the cold affusion produced these effects. The tepid affusion was less permanent in its effects, as you have observed; for in no case did it produce a total cessation of the fever, although it always alleviated the symptoms very much, and diminished their violence: the cold affusion generally arrested the progress of the fever when it was employed early in the disease; but the cutting short of the fever did not always follow the reduction of the pulse and the diminution of the heat. In one case the heat of the body was reduced $4\frac{1}{2}^{\circ}$ by the cold affusion applied in the evening; and in another, the pulse reduced thirty beats in the minute, without the disease being stopped. It appeared that the flow of the menses was interrupted by the tepid affusion, they returned on discontinuing its use, and no inconvenience followed."*

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This account obligingly given me by Dr. Reeve, of Norwich, corresponds with communications made to me on the

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same

The cold affusion has been employed in fever with success in Glasgow, and several other parts of Scotland, but the details I have received relate to single cases only, which it would be useless to insert.

From Dr. Bree, Physician to the General Hospital, and to the Dispensary of Birmingham, I have received a clear and valuable report, on the condition of the poor in that great manufacturing town, as far as respects health and disease. The limits to which I am confined prevent me from inserting it entire, but I hope it will in one form or other be presented to the public; in the mean time I must content myself with that part of it which more immediately refers to the subject of this publication.

Birmingham, from the form of its buildings seems less exposed to infectious fever than Liverpool, or most other great towns. The poor there do not inhabit cellars, and the courts are more spacious and better ventilated than ours. Fever

same subject, by my friend, and kinsman Dr. Thomas Duncan, and by Dr. Bouchel, of Ghent, both of whom attended the clinical ward, when conducted by Dr. Home that winter (1802-3). I hoped to have had a very full and precise communication from Dr. Home himself, but an accidental circumstance has occasioned me to be disappointed.

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occurs there however as every where else, but it was more especially prevalent in the years 1799 and 1800. In the winter of those years, the remote causes of fever existed generally in the dearness of provisions, and want of employment for workmen. Dr. Bree points out how these operate in a detail which appears equally just and affecting. The fever originating in defective nutrition and cloathing, depression of mind, and contaminated air, was often preceded by purging and vomiting, immediate effects of debility in the first passages; when these were restrained by medicine, fever very often ensued. "The fever," says Dr. Bree, "was not very different from that which you have described. When delirium did not appear at the very first, it came on about the seventh day, after great pain of the head, and in two days after it was followed by stupor and deafness. Then the patient frequently began to recover, but sometimes his brain was greatly affected, and he grew worse. The well-known signs of the most dangerous state of typhus appeared, and death was the result. In these cases the pulse was sometimes not more frequent than 100 in the exacerbation, but unless the brain was particularly diseased, the pulse increased in frequency towards the close. The heat was from 100° to 110° Faht. in the same circumstances. In many cases the symptoms

were very similar to those that distinguished the fever described by Sydenham, sect. 5. chap. 8, particularly as regarded the pain and stupor of the head, and rheumatic pains in the breast and neck. In these, sweats appeared to be critical after the 14th day, and then a disposition to sweat continued for several weeks coming on in the night. I found in several instances that checking the perspiration under such circumstances was injurious.

“ This fever continued in 1801, though rather on the decline. It particularly affected pawn-brokers, whose custom was to take the cloaths of the miserable victims, in pledge for a small loan to furnish them with immediate subsistence; these cloaths were of course often infected.” Dr. Bree enters very particularly into the distress; moral and physical, which contributed to this disease. “ Many families had subsisted on barley and potatoes, in scanty portions, for many months, frequently without even a pound of butcher’s meat for their Sunday’s dinner. I have seen not unfrequently beans boiled with salt and water into a soup, that served for food during several days.” After entering into a variety of particulars of this kind, Dr. Bree proceeds as follows. “ If you consider the above sketch necessary to distinguish the typhus

phus here, as influenced by our internal circumstances, you will excuse this detail. It is a more pleasing task to speak of a simple and efficacious means of prevention, or cure, when the constitution is not deprived of that support of food which is essential under every plan of medical treatment; this remedy is the aspersion of cold and tepid water. It will be obvious why I have particularized the difficulty of subsistence, and the moral causes of distress. No estimate could be made of the value of a remedy, if the counteracting influence were not calculated.

“ There was only one variety, which according to my observation would not receive with great advantage the aspersion. This was where the patient had rheumatic pains in the muscles of the head and neck, and in these instances I found advantage in the suggestion of the practice, as it led me to recommend the cleansing of the skin with tepid salt and water, but not in a way to give a shock.

“ I have sometimes used the aspersion when a high accompanied the fever, and though I was timid in this practice, I have reason to think that it was beneficial. Under the circumstances that I point out of bodily heat, in general cases of

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typhus,

typhus, I have found excellent effects from aspersion of cold or tepid water. The advantages seemed to be combined, the heat of the body was reduced to 97° or under, and the skin was rendered more clean and healthy, and therefore more capable of transpiration. When the heat was steadily above the temperature of health, cold water was applied without hesitation, and it seldom failed to give tranquil feelings, sleep, and perspiration, and to reduce the pulse. It was often the most active remedy in families of from five to eight persons, who in consequence of a want of recommendations, had only partial assistance from the medicines of the Dispensary, whilst the disease appeared to affect the whole.

“ When the fever had been protracted beyond the fourteenth day before I saw the case, I judged it most prudent to apply tepid water, and that without a shock in most cases, but the cleansing effects were in my opinion, in all cases beneficial. In several instances, I am convinced that the fever was extinguished by cold affusion only. These were chiefly cases of children that had received the infection from their parents, who probably had taken care of their offspring at the expense of their own support. I could offer many details of this practice in particular cases, but as I adopted your directions and reasonings, only

only modified according to local circumstances, it would be useless to take up your time with such details.

“ The internal remedies for the fever were, bark, and the usual stimulant cordials. We seldom saw the disease at the period when antimonials are useful. Cold drinks were generally directed, but cold water was not so distinctly useful as a drink, as in external application.

“ In addition to the tonic and stimulant class of medicines that are well known, I employed a medicine, which after much experience of its effects, I can recommend as a powerful and cheap substitute for the bark, or any combination of bark with other stimulants. It was prepared as follows.

“ Take oak bark bruised, and roughly powdered ; horse radish roots sliced ; of each, an ounce. Boil the oak bark in two pints of water, till one pint be consumed. Add, before the boiling is finished, the horse radish. Then cover the vessel till the decoction is cold, and strain it for use. Sometimes elixir of vitriol was added to this, and if purging was threatened, or general uneasiness was felt, thirty drops of tincture of opium were added to each pint. Two ounces were given.

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every four or five hours, and this medicine, with the aid of aspersion of water, or of washing the skin with vinegar and water, or salt and water, where the full effect of cold bathing was improper, was attended with success in many of the worst cases that occurred, and after remedies of the most established credit had entirely failed."

" *Birmingham, Feb. 1803.*"

That the affusion of cold water has been employed at Norwich has already appeared from the communication of Mr. Dalrymple. It was previously employed by Mr. Martineau of that city, as appears by his valuable communication in the third volume of the Medical and Physical Journal, p. 51. I have the pleasure of learning from Dr. Reeve, that this ingenious practitioner continues the use of this remedy, and has lately extended it to Scarlatina with great success.

I have much satisfaction in finding that the method of treatment of fever which I have recommended, is gaining ground in the military hospitals of our island, where I have no doubt it will soon be completely established. The following narrative of Mr. Marshall, surgeon of the royal Cheshire Militia, contained in a letter addressed to me, which has already appeared in the Medical and Physical Journal for November,

1801, has essentially contributed to extend its progress, and deserves in every point of view to be re-published.

To Dr. CURRIE.

“Liverpool, July 20, 1801.

“ Sir,

“ In the month of May, 1800, the Cheshire regiment were in barracks at Gosport, when the typhus fever made its appearance among them. It was probably communicated to them while on duty over the French prisoners, among whom it prevailed. The first symptoms were a dull head-ache, with restlessness and shivering, pains in the back, and all over the body, the tongue foul, with great prostration of strength. The head-ache became gradually more acute, the heat rose to 102° and 104° , and in one instance 107° ; and in general the restlessness increased to delirium, particularly in the night. The fever spread with rapidity. At first we employed the usual remedies; emetics in the first instance, anodynes for a day or two afterwards, to keep the skin soft, then wine, in proportion to the debility, from a pint to a quart and upwards, with nutritious diet, and an opiate, consisting of 40 or 50 drops of the tincture of opium every

every night. The bowels were kept open with calomel and rhubarb, and barley water and lemonade were drunk at pleasure. In four cases I gave three bottles of wine a day. These seemed desperate, but all of them finally recovered. Port wine seemed scarcely to stimulate in these instances at all; I changed it for Sherry, and this I sometimes mixed with brandy. In a single month the wine cost me ten pounds four shillings, though I had a great part of it from the mess, where it was laid in in quantity, and on the lowest terms. I used few blisters and no bark. This last medicine I had given before, in a number of similar cases, at Winchester, without any benefit. Where there was delirium, which was pretty general, the head was shaved. This practice I continued for two months; during that time thirty of the men were seized with the infection, and in few or no instances was the disease stopped by the emetics or antimonial sudorifics. The fever ran from thirteen to seventeen days, and in some cases to three or four weeks. We lost during the fever one man only, but several from the effects combining with other causes, particularly pulmonic consumption.

“ The contagion continued to spread in spite of all our endeavours. At length we had twenty-five

ve in the hospital together, in the different stages of the disease.

“ Finding none of the usual means successful in arresting the fever, I had recourse to the affusion of cold water; this was towards the end of July. The first case in which I tried it was that of a battalion man; in the second day of the disease, I stripped him naked and threw four or five gallons of sea water over him, dried him, and put him to bed. He felt very comfortable, became drowsy, and slept for two hours; when he awoke his skin was cool and moist, the pulse nearly natural, and the head-ache in a great measure gone. In the evening the fever returned; I threw the water again over him with the same happy effect. He had a good night's rest; next morning he was free of fever, and he was dismissed cured, on the third day from his admission.

“ Pleased with this success, I immediately adopted the same practice in nine other cases of fever, from the first to the fifth day of the disease, with equal benefit. In six other cases, from the sixth to the tenth of the disease, I followed the same practice; in three of these with complete and nearly immediate success, having used affusion only thrice with each. From the debility however which had been induced by the
the

the longer continuance of the disease, they were not so immediately fit to be discharged from the hospital. In the three other cases sensible relief was obtained, and they all recovered in the end, but the disease ran its course. Encouraged by the success and safety of the practice, in one instance I went so far as to try the cold affusion on the sixteenth day of fever. At the moment, the patient seemed almost insensible to the shock. When he was replaced in bed, his extremities and his surface were cold; the pulse became fluttering, and scarcely perceptible. We used frictions, and poured warm wine into him in small quantities at a time, which he swallowed with great difficulty. He recovered his power of deglutition, and we increased the wine gradually to three bottles a day. He recovered in the end, but was upwards of three months in the hospital.

“ In a few weeks, the old cases that were on hand when I began the use of the cold affusion were discharged, but the contagion, from the nature of the duty, continued to spread in the regiment, and many cases of fever occurred in the months of August and September. These were watched narrowly, the cold affusion invariably used, and in general on the second day. The effects were similar to those related of the case
first

first mentioned; the success was invariable. One of these cases, that of Holding, of the grenadiers, was remarkable. He was taken into the hospital on the 20th of September, late in the evening of the second day of fever, with the usual symptoms, but in the severest form. An emetic was administered, and the affusion deferred till morning. The emetic operated well, but his night was extremely restless, his head-ache particularly acute, and delirium came on with great violence. It was necessary to employ force to keep him in bed. In the morning of the 21st, (the third day of fever) I found his heat had arisen to the uncommon height of 107° , and his pulse was 125. The cold affusion was employed; he screamed from the severity of the shock; but, on turning into bed, he appeared much refreshed, was perfectly sensible, and said that he thought himself well. On examining about ten minutes after the affusion, the heat was found to be 100° , the skin moist, the pulse 110. He slept for nearly four hours in perfect tranquillity. In the evening the fever returned. The affusion was repeated again with similar benefit. He slept the greater part of the night. Next morning the fever returned once more. The remedy was once more applied, and the disease was subdued. He was dismissed from the hospital on the 29th.

“ By

“ By this time the hospital was thin from the rapid discharge of the patients, but the contagion still continued to spread in the regiment. The cold affusion was invariably applied, and, in general, on the second day of fever, as has already been mentioned. At length the contagion was extinguished; when we left Gosport, in the month of November, not a single case of fever was left behind. From the end of July to the 31st of October, I employed the cold affusion in sixty-four cases. In sixty of these I arrested the disease, having seldom occasion to use the remedy more than twice or thrice, and in no one case more than four times. In the other four cases, (all of which are alluded to in the course of this narrative) the disease being advanced was not stopped by the remedy, though the patients ultimately recovered.

“ From the time I began the use of the cold affusion, I used little or no wine, no opium, no indeed, almost any other remedy, in any one case in which the cold affusion was employed.

“ A great part of these facts was witnessed by Dr. Franklin, physician to the district; nearly the whole of them by my fellow-surgeons, Mr. Varenne and Mr. Worthington; and the general

truth

truth of this representation will be confirmed by my friends, the officers of the Cheshire, particularly by Major Buckworth,* who frequently visited the hospital. I have the pleasure of adding, that any evening on parade, I can point out the individuals whose cases I have mentioned.

“ I have the honour to be,

“ Your very obedient servant,

“ JAMES MARSHALL,

“ *Surgeon, Royal Cheshire Militia.*”

Any comment that I could make on this narrative would only weaken its effects. Experiments such as these, so clear and simple, on so large a scale, and performed before so many witnesses, cannot, it should seem, be invalidated. Mr. Marshall's practice was in exact conformity with the directions in the “ Medical Reports.” It was the low contagious fever that his patients laboured under; it was in the early stage of the disease that he employed the remedy, and generally in the state of the greatest heat and exacer-

* This spirited and accomplished officer, had called on me immediately after the Cheshire regiment marched into Liverpool, (a little before the date of this letter) to introduce Mr. Marshall to my acquaintance, in order that he might communicate the above important particulars.

bation;

bation; lastly, it was affusion, not immersion, that he employed.

I might mention similar instances of success in other regiments, but not on so good authority, or such complete information. I shall not therefore allude to them farther, except to express a wish, that those who have withheld the particulars from me, will find some other means of laying them before the world.

I might publish a number of individual cases of the success of the cold affusion, chiefly communicated to me two or three years ago, by respectable practitioners in this neighbourhood, and in other parts of England; but the results are so uniform, and the circumstances so much alike, that I have thought it now unnecessary to present them to the world, and must content myself with offering my thanks generally, for the kindness and zeal, of which, had I returned to the press earlier, I should undoubtedly have availed myself. I ought not, however, to omit mentioning the frequent intimations given by Dr. Reid, of the use of the cold affusion in the practice of the Finsbury Dispensary.* That re-

* In the Monthly Magazine, and Medical and Physical Journal.

spectable physician has lately extended the use of this remedy to scarlatina with great success*.

I have only to add, under this head, that the use of the cold affusion has been introduced into the Isle of Man, under the skilful direction of Dr. Scott, who has found it in some instances advantageous to precede it by an antimonial emetic.

I have no particular accounts of its progress in Ireland, where it was had recourse to by a very particular friend of mine, not of the medical profession, three years ago, in the case of a brother of his, dangerously ill of fever, with the immediate removal of delirium, and every wished for effect†.

* See the Medical and Physical Journal for January, 1804, p. 27.

† In the additional chapter to this (fourth) edition, (chap. 1.) some account is given of the farther progress of the practice in question in Ireland.

CHAP. IV.

Some Account of the use of the cold affusion in fevers on Ship-board.

VARIOUS histories are to be found in the records of medicine, of persons, who under the delirium of fever have thrown themselves into cold water ; in almost all of which it is mentioned as very extraordinary, that the patients, when taken up, were perfectly in their senses, and speedily recovered of the disease. Of these cases a great number have happened at sea, where it is evident accidents of this kind are most likely to take place.*

I have

* In a work on the diseases of seamen, published by Dr Cockburn, physician to the fleet, about the beginning of last century, after relating a case from Dr. Willis, of a young woman under fever, who wished to go a swimming, and who being indulged, afterwards recovered, the author adds
 " Some such like cases happen very often to our sailors, wh
 " "

I have had authentic accounts of several accidents of this kind, which have happened on board of ships belonging to this port, within the last twenty years, and in every instance where the patient has been recovered from the sea, he has been found in a great measure free from fever. From the general views we have taken of the effects of the cold affusion or immersion in fevers, such results might be expected. Incidents of this kind do not occur under the low delirium, towards the termination of fevers, when the strength is exhausted, and the heat reduced, but in the earlier stages, under the high delirium, when the heat and agitation are great, and the patient is still possessed of the power of voluntary loco-motion.

It does not however appear, that in the maritime practice of medicine, of this, or of any other nation, advantage was drawn from these

the time that they are delirious, or have calentures, when lying in their hammocks in a calm summer's day, they see the sea through the gun ports, very plain and smooth, and imagining it to be a green meadow, get up and walking, and fall into the sea, but if they get at last into their hammocks, (i. e. if they are taken up alive) they sweat very plentifully, and shake off their fever." See *Ména Naut.* v. iii. p. 27. *

extraordinary recoveries, till the voyage of Dr. Wright from Jamaica to England in the year 1777, with the account of which this work commences.

Immediately after the publication of this important narrative, the use of the cold affusion in fevers was employed externally (as has already been mentioned) in Liverpool, and especially in our fever wards in the Infirmary, where it became familiar to the pupils of the house, and to various medical practitioners engaged in the African trade, or destined for the West Indies. By several of the African surgeons, the practice was adopted with success, but the accounts which I have received from them are not sufficiently particular for publication.

Mr. Wilson, Surgeon of his Majesty's ship the Hussar, employed the cold affusion with extraordinary success in 1795. The Hussar had taken a French ship from Guadaloupe off the Capes of Virginia, which had brought the yellow fever out of port with her, of which many had died. The disease spread rapidly among the crew of the Hussar, and the ship running northwards, landed eighty-three persons ill of the fever at Halifax, in Nova Scotia, in the month of June. There not being accommodation for them

them in the hospital, tents were fitted up for them on shore, in which Mr. Wilson attended them. His practice was to bleed early, generally in an hour after the accession of the hot stage. He then gave a solution of emetic tartar. The cold affusion was always administered in cases of delirium, which it immediately removed, inducing tranquillity and sleep. Of these eighty-three cases, Mr. Wilson did not lose one*.

I regret that we have not a more particular account of the practice of Mr. Wilson. In the burning fever of the West Indies, especially when it affects youthful and vigorous Europeans, as in the case of the crew of the Hussar, it seems to me very probable, that early bleeding, followed perhaps by antimonials, may sometimes be advantageously employed preparatory to the cold affusion; and that the inordinate action of the vascular system being weakened by these previous steps, the cold affusion may afterwards be more speedily effectual. But this must depend on the vigour of the patient, and the effects produced by these remedies on his temperature; which in no case ought to be reduced so low as the standard of health, previous to the cold affusion, and it

* *Med. Nautica*, vol. i. p. 361.

seems also clear, that there is no safety in the use of blood-letting or antimonials, except they are had recourse to in the very first stages of the disease, and in patients in the vigour of life, as in the practice of Mr. Wilson of the Hussar. There is little doubt that this gentleman was partly indebted for his extraordinary success, to his attending his patients in tents, where they must have been much exposed to the open air, a circumstance which at Halifax, in the middle of summer, must have been no less pleasant than salubrious.

A practice similar to that of Mr. Wilson, was employed by Mr. Harris, Surgeon of the 'Thunderer.' He also bled in the first stage of fever, and gave clysters of cold water, which were very advantageous*. Experience has taught me, that the heat of the system may be effectually reduced by clysters of cold water, though not so speedily as by affusion on the surface, or even by deglutition.

Previously to the publication of the "Medical Reports," Dr. Trotter, Physician to the Channel Fleet, had recommended the cold lava-

* Med. Nautica, vol. i. p. 860.

tion after the manner practised at that time by Dr. Gregory. His directions were, that every morning, a towel dipped in cold water should be passed over the surface of the patient, and afterwards that his linen should be changed*. After the publication of this work, he encouraged the use of the cold affusion, as recommended in it, of the effects of which he has given several striking and satisfactory accounts. Some of these I will take the liberty of extracting.

In June 1800, the cold affusion was employed by Mr. Farquhar, Surgeon of the Captain ship of war. His account of the effects are as follows :

“ In several cases of typhus which have occurred this month, I have made use of the affusion of cold water, and have found it to answer my most sanguine expectations, particularly in two cases where delirium had come on, and where, from the general debility, frequency, and irregularity of the pulse, great heat of the skin, &c. I had little hopes of recovery. The patients were put into a large tub, and had a couple of bucket-fulls of salt-water poured over

* Med. Nautica, vol. i. p. 279.

them. The shock which they at first received, appeared to be very severe, but the advantage which they derived from the practice was so great and manifest even to themselves, that they willingly submitted to its repetition next morning. They are both at present convalescent*.”

In spring 1800, the Russell man of war, commanded by Captain Sawyer, was dreadfully affected by contagious fever, which after being subdued, broke out again in the harbour of Plymouth, in consequence as it was supposed, of fresh infection received probably from the impressed men. The disease spread with great rapidity, and great numbers were affected. In this epidemic, the cold affusion was employed by Mr. G. Magrath, the Surgeon, with the happiest effects. In a letter to the physician of the fleet†, he sums up his experience of this remedy in the following words:

“ I have now had ample experience of the effects of cold affusion in fevers, and from my own observation, will venture to pronounce it a safe and efficacious remedy, when the restric-

* *Med. Nautica*, vol. iii. p. 162.

† *Med. Nautica*, vol. iii. p. 237.

as so judiciously laid down by Dr. Currie are
 ended to. I can affirm that no evil of any
 magnitude ever arose in the course of my prac-
 tice from its adoption, *even when catarrhal
 symptoms were present.* I have experienced,
*that the earlier this remedy is had recourse to in
 the disease, in like proportion it will be found
 to be effectual in arresting the progress of fever."*

Several cases have yielded to the cold
 application, without the assistance of any medicine
 whatever; but I have found, that the prudent ad-
 ministration of evening anodynes, powerfully as-
 sisted. In the more advanced stages, where the
 skin was dry and impervious, with more or less
 fever, and where a cuticular discharge (sen-
 sible perspiration) could not be procured by the
 most powerful sudorific medicines; by throwing
 a bucket of cold water over the patient, rub-
 bing him dry, and laying him between the blan-
 kets. I found a comfortable glow to succeed,
 followed by a gentle and salutary perspiration,
 first appearing about the forehead and neck;
 and by administering bland diluent drinks, such
 as lemonade, or sage tea, this perspiration would be-
 come general, the confusion of thought disap-
 pears, and the patient would fall into a calm and
 refreshing sleep, which sometimes lasted for
 hours.

hours. He would then awake much recruited with an abatement of all the febrile symptoms and in several a complete recovery took place under these circumstances.

“ Indeed, *invariable success has attended my endeavours ever since I began to adopt this practice on a large scale.* It is now so perfectly familiar, that I have the utmost confidence in it; and feel justified in pursuing it from principles of humanity as well as of science. In some of the milder cases, I have known one bucket of water poured over the head and body produce such a shock, that the redundant heat and increased velocity of the circulation disappeared and never more returned. The disease was this means cut short, which otherwise might have been protracted to an uncertain duration with increasing malignity.”

The method of cure thus described by Mr. Magrath, was pursued by him in the harbour of Plymouth, and drew on him the attention and observation of the Officers, as well as Surgeons of the fleet, but he did not shrink from his course, and his triumph was in the end complete. Mr. Magrath was supported by Captain Sawyer and his Officers, whose attention and

y towards the sick, kept pace with the able and benevolent arrangements of the Surgeon*.

In consequence of a notice which I inserted in the Medical and Physical Journal, inviting particularly military and naval practitioners, to pour me with the account of their experience in the practice in fever which I had recommended, I received an ample and valuable communication from Mr. Magrath, dated, *his Majesty's Ship, Amphion, Portsmouth, April the 10th, 1803*, to the whole of which I would willingly give insertion here, if my limits would permit. Mr. Magrath has enlarged his experience of the cold affusion, since the date of his communication to Dr. Trotter, and confirmed inferences.

"My first trial," says he, "of this remedy, was the consequence of multiplied disappointments in the ordinary modes of practice, and was resorted to with no little circumspection, on account of the inveterate prejudices of such a class of men as sailors are; but by steady perseverance, and dispassionate investigation, the happy effects of the cold affusion were incontro-

* Med. Nautica, vol. iii. p. 237.

vertibly developed, to the conviction of those on whom the remedy was used, and who were as anxious for its repetition at the proper periods, as I was to direct it.

“ I have now put this remedy to the test of accurate trial, in upwards of a hundred cases of typhus, differently modified by climate and season, and diversity of constitution, sometimes complicated with catarrhal symptoms, and sometimes with affections of the bowels, not only in the English Channel, but in the Baltic sea, and in the increased temperature of a southern latitude: and *with invariable success*. When employed in the first stage of fever, it frequently happens, that a single application is sufficient, and I have always observed, that it is more powerfully efficacious, in proportion as it is early resorted to, and that if neglected till the morbid catenation is strongly formed, the fever frequently resists the cold affusion for some time.”

Mr. Magrath laments, that the cold affusion has not been adopted generally in the navy, and expresses his indignation against persons who have aspersed and reprobated it without a trial, on mere prejudice. He anticipates the time as at no great distance, when it will be firmly established. In the

st sentiment I perfectly agree; and in regard to the opposition of ignorance and prejudice, we shall bear it with more patience, when we recollect that it has so uniformly presented itself to all considerable improvements in our profession, that we might begin to doubt of the great advantage of the practice we are recommending, if it wanted this genuine mark of utility and of importance.

In the course of his letter, Mr. Magrath adverts to the great number of ships connected with the channel fleet, that in the course of the spring and summer of 1800, “were contaminated with phus contagion!” and considers this contagion to have originated in guardships, tenders, and receiving ships. The same sentiment is delivered by Dr. Trotter, in his valuable record of the diseases of the fleet—the *Medicina Nautica*, already so often quoted.

Dr. Trotter points out the Actæon, the receiving ship at Liverpool, as in this respect particularly obnoxious, and denounces the impressed men sent round to Plymouth from her, as having spread contagion through the channel fleet. I fear there is too much ground for Dr. Trotter’s charge. In availing to the Actæon, he has mentioned as having on one occasion been called to examine the condition of the men sent from this receiving ship,

ship, and as the particulars may serve to illustrate our general subject, I will give them here in detail.

One of the vessels sent to convoy the impressed men from the Actæon to the channel fleet, was the Reynard sloop of war, commanded by Captain Spicer. She performed three voyages on this service in the course of the spring and summer of 1800. The first cargo which he carried round, having been infected with fever, and having, as it was supposed, spread it widely in the fleet, Captain Spicer was anxiously desirous on his second voyage, to take no seamen from the Actæon that were not in perfect health. His orders, which he communicated to me, and the commander of the Actæon, were very express to that purpose. Accordingly, about an hundred men, supposed to be free of infection, were sent on board of the Reynard; but a contrary wind preventing Captain Spicer from sailing immediately, he perceived, or thought he perceived, febrile symptoms on some of those men, two or three of whom he returned to the Actæon instantly, and dispatched a summons to me on shore, to repair on board the Reynard and examine the rest.* I repaired on board accordingly

* I was unacquainted with Captain Spicer and no wise connected

rdingly, along with the gentlemen who super-
tended the department of the sick and hurt,
d the impressed men were passed in review be-
re us. A slow contagious fever had got among
em, and eighteen, who appeared to be affected,
ere sent to the hospital on shore. Captain Spicer
iled with the rest, but all our care had not
cured him from the apprehended evil—four men
ere found ill of the fever on their passage, and
nt to the hospital on the ship's arrival at Ply-
outh.

The *Actæon* deserved the character given of
r by Dr. Trotter; she was indeed a disgrace
the service in various points of view. After
his time, however, greater regard was, I believe,
aid on board of her to cleanliness and ventila-
on.

The men sent on shore were attended by Dr.
omas Cochrane, formerly of the island of
evis, and now resident in or near Edinburgh,
no had at that time the care of the French pri-
ners, and of the sick and hurt at this port.

nnected with the service; but no man could participate
ore fully in the anxieties of this brave officer, or obey his
ll with greater alacrity.

His

His practice was to employ the cold affusion, and it was used in the case of these men with its wonted success. This fever, which is said to have spread itself through the fleet, yielded readily to this simple remedy. My friend, Dr. Carson, now physician in Liverpool, assisted Dr. Cochrane, and on some occasions acted for him. He did not attend the individuals in question, but he has favoured me with the following particulars, which are important in themselves, and from which the successful practice of Dr. Cochrane may be clearly inferred.

“In the month of December, 1799, an epidemic fever appeared on board his Majesty’s ship *Actæon*, stationed at this port to receive the newly raised men for the navy. My friend Dr. Cochrane, who at that time had the charge of the naval hospital on shore, was a great advocate for the use of the cold bath in fevers and spasmodic complaints. I had at all times permission to visit the hospital. An occasion offered which I considered as favourable for using the cold bath, and I obtained Dr. Cochrane’s permission to try the effects of a remedy, to which he himself had, in such cases generally and successfully had recourse.

“Five men had been sent on shore, who had been seized with the epidemic the preceding day.

About

About nine o'clock at night, I found several of them in a state which, from attention to your rules, I considered proper for the application of the cold bath. They complained of great heat and thirst. Their skin was dry and hot, tongue parched, great restlessness, with occasional delirium; pulse quick, respiration anxious, though without any cough or local affection of the breast. I did not venture to apply the cold bath to them all this evening; but selected one whom I deemed the fittest subject for it. This man, having been got out of bed, and placed in a large tub, two buckets full of cold water were poured over him. He was then put to bed; nothing further was done to him. In the morning I returned at an early hour, anxious about the success of the practice, and had the satisfaction to find him free from fever, and to understand that he had passed a comfortable night. He had no relapse, and in a few days was returned to the Actæon in good health. The other four in whom the fever continued its course that night without abatement, were next day, in the presence of Dr. Cochrane, treated in the same manner, and with exactly the same success. The remedy was most grateful to the feelings of all these men, and there would have been no difficulty in persuading them to use it a second time, had there been occasion for it. This was the general practice with the fever cases from the Actæon.

"I have frequently seen the cold bath used in cases of fever, among the prisoners at war deposited here in the latter end of the late war, and always with complete success. But in the prison-hospital, fevers were, from obvious causes, too frequently attended with pulmonic affections to admit of the use of this admirable remedy."

Dr. Carson has also communicated to me the following general testimony of the benefit of the cold affusion, in a letter to him from Mr. Simpson, Surgeon of his Majesty's ship *Naiad* :

*"Ship Naiad, at sea,
June 22, 1803.*

"I have for a long time past been in habits of employing cold water externally, although in a different, and certainly far less extensive form than Dr. Currie recommends. And indeed, I do not know, who had any idea before his valuable communications, of the vast extent to which this scientific practice may be carried.

"In every case of typhus, that for a long time past came under my care, I have always assiduously put in execution the application of vinegar and cold water, by a sponge or towel, to the head, neck, and breast, regularly every morning and

and evening, and in cases where debility was not far advanced, I have perhaps used it oftener. And I can with truth aver, that I have succeeded in a great number of instances, in arresting fever at a very early stage by a timely use of the cold affusion, as recommended by Dr. Currie. Indeed so successful have I been, that in all my reports for these last few years, I have scarcely had occasion to note any continuance of fever for any remarkable time, although I am well convinced, that if I had not practised the cold affusion, as recommended by him, I should not have been nearly so successful.

"I have conversed with a number of ingenious men on this subject, and they have unanimously concurred in giving the strongest testimony in favour of this excellent practice.

"I certainly feel authorized in saying, that fevers, which from every rational consideration, it could be adduced from previous knowledge, are likely to prove tedious, if not extensively fatal, have by means of the cold affusion solely, far as I could judge, been happily arrested, and terminated favourably.

In almost every stage of fever, I have found the remedy useful, when prudently employed.

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When early put in practice, that is to say, as soon as the fever is properly formed, I never have found it fail of affording material relief; and generally a few applications have actually put an entire stop to the farther progress of the disease.

“In a few instances however, when from the state of the pulse, skin, &c. I had apprehended, I should be warranted in using the aspersion with cold water from a jug, I found that this did not succeed so well as the simple ablution with a sponge. Very severe and long continued rigours followed the aspersion from a jug, and they did not terminate, as I had always found in other cases, in sleep, and a genial diaphoresis, nor was there the least abatement of head-ache. But this might proceed from the patients terrors at the sudden and unusual application. For certainly, until Dr. Currie’s practice became generally known, this application, the aspersing a person in fever with cold water, was quite repugnant to the generally received opinions, and practice of mankind—at least I believe so.”

These testimonies in favour of the cold affusion, seem decisive of its beneficial effects in the maritime practice of our northern latitudes. I

it were necessary, it is in my power to add to them considerably. But candid inquirers will be more desirous of knowing how far it is equally salutary in the destructive fever of the West Indies, by which our fleets and armies have been so often enfeebled or subdued.

In consequence of my address in the Medical and Physical Journal already mentioned, I received a communication from Mr. L. F. Nagle, lately surgeon of his Majesty's ship Ganges, which bears on this point; and which appeared to me so very important in its nature, that I was desirous of giving it to the world with every stamp of authenticity: and Mr. Nagle having referred me for his character to Captain Freemantle, and Captain Baker, who commanded the Ganges, during the period of his service on board of her, I applied by letter to the first of these gentlemen, the address of the other not being known to me. The reply of Capt. Freemantle, dated March 17th, 1803, was perfectly satisfactory. He spoke of Mr. Nagle in the highest terms; represented him as no less honest than skilful, and assured me, that I might place the most perfect reliance on his representations.

Satisfied with the high authority of Captain Freemantle, and with Mr. Nagle's own perspicuous

cuous and unaffected narrative, I present it to the world with perfect confidence, and with singular satisfaction. It was originally contained in a series of letters, and reduced to one unbroken narration at my request.

Copy of a Letter from Mr. Nagle to Dr. Currie.

*" His Majesty's Ship Royal Sovereign.
" Portsmouth, May 9th, 1803.*

" Sir,

" I took the first moment after my return from the West Indies, to acquaint you with the extraordinary success which has attended your new remedy in fever, the affusion of cold water, in my practice on board his Majesty's Ship Ganges, of 74 guns, on the Jamaica station; and I very readily comply with your desire to give the particulars a little more in detail.

" The Ganges was commissioned at Portsmouth in August, 1800, at which time I joined her as surgeon. She was then, and for some time afterwards remarkably healthy, from her high state of discipline; but an infectious fever was brought on board of her by some marines, from the Malta,

at

at Spithead, in August, 1801.—Forty Marines, and four seamen, ill of it, were sent on shore to the Royal Hospital, at Haslar, and this cleared the ship of the disease. We then sailed for the coast of France, and were on a cruise off Brest, from the 14th of September, to the 23d of October, at which time we were ordered to Jamaica, where we arrived on the 24th of November. During these ten weeks not a man died, and we reached the harbour of Port-Royal in remarkable health. We found a malignant fever prevailing among the shipping there. The mortality was particularly great on board the merchant ships at Kingston, many of them being almost unmanned by it. Soon after our arrival it broke out on board the *Ganges*, and spread rapidly, especially among the marines and landsmen, who had never before been in a warm climate. The symptoms were—severe head-ach;—hot and dry skin;—the face flushed;—eyes red;—nausea;—thirst;—the pulse strong, and full at first, and as frequent as 120 in the minute;—pains in the back and limbs;—great anxiety, and restlessness. The patients were in general under much depression of spirits from the accounts we had received of the great mortality from the fever. There was little chilliness in any stage of the disease, and remissions were scarcely perceptible. Heat of the skin was the most striking symptom. To what degree it actually rose, I had

no means of ascertaining, having unfortunately, broken my thermometer. I was therefore obliged to trust to my sensations, and those of the patient. But from the impression on my hand, I have no doubt that the heat in this fever, was many degrees greater than the temperature of health, and considerably more than in the common fever of England.

The violent, and rapid nature of the disease, convinced me, that early and decisive measures were required, and I determined to have recourse to the affusion of cold water, under the directions which you have given for its use. As soon therefore as the morbid heat fairly indicated the accession of fever, I poured a quantity of sea water on the patient, from the head downwards; generally two or three bucket-fulls; and commonly directed the body to be afterwards wiped with a towel dipped in vinegar, but more with the view of preventing the sailors from thinking that I trusted entirely to the cold water, than from any supposition of the vinegar being required. I then put the patient into bed; gave him in general from eight to ten grains of calomel, with four or five grains of the pulvis antimonialis, and supplying him with plenty of diluent drinks, left him to his repose.

“The affusion, when used in the first, or even second day of fever, operated like a charm. The torrid heat and dryness of the skin were converted into an agreeable coolness, with some degree of moisture; the pulse sunk very often from 100 to 90; the headach, flushings, restlessness, and agitation disappeared; sensations of comfort were diffused over the whole body; and the patient fell into a natural and refreshing sleep. On making, two or three passages downwards from calomel, seemed to carry off every remaining irritation. Most commonly the fever did not return; but if it did, the bathing was repeated once, or perhaps twice, as might be required. Where I had not an opportunity of seeing the patient for the first day or two of fever, which sometimes happened from their being taken ill on shore, and remaining there, the effects of the affusion, though strikingly beneficial, were not immediately decisive, and it was requisite to repeat it several times. I had seldom occasion to give opium in this fever, for the cold affusion produced sleep, and in three or four cases in which I gave opiates at bed-time, irritation and restlessness ensued; the symptoms being encreased which the cold affusion had obviated. We had one hundred and twenty cases of fever in all, during the time I served on board the Ganges, on the Jamaica station, that is, from November, 1801,

to the end of July, 1802) in all of which the cold affusion was used, and of which we lost two only. One of these had been ill of a violent inflammation in the knee, for which I was obliged to use bleeding largely, and in this reduced state he was attacked with the fever. The other was a marine, of a weakly habit, and a consumptive tendency. I did not use the affusion in this case, in the early stage of the fever, and when I did use it, I fear I persisted in it too long. These were indeed the only deaths which occurred on board the Ganges from the time we left Portsmouth, to the end of last July, a period of eleven months, as may be ascertained by my journal at the board of Sick and Hurt, and during this time, two men only were sent to the hospital at Port Royal, one from an accident, the other from general debility.*

“ The

* “ I had a great objection to sending patients to the Hospital at Port Royal, from the air at sea being far preferable in the West Indies for fever patients. The mortality at the hospital is often very great, and many other cause prevent men from returning to their ships again. Captain Freemantle was so sensible of this, that he would not allow a man to be sent on shore, except in cases of the most urgent necessity, knowing the great loss to the service that good men are during the season of war. If proper diet is allowed by government, their recovery is more rapid at sea, and taking

“ The fever broke out first at Port Royal, as I have already mentioned, and we carried several cases to sea with us, on a cruise to the island of St. Domingo, on which we were out six or seven weeks; when we repaired to Port Royal to refit and refresh for a few days, and again returned to our cruising ground. In these successive cruises, our whole time was employed. We had a great deal of calms, with heavy rains, at intervals, off St. Domingo, and at this time the mortality was understood to be very great among the French troops on shore. While at sea, we were often, for several weeks together, not only without fresh vegetables, but without fresh animal food, and without even a single glass of lime-juice, to correct the scorbutic tendency in the men. Many of my patients in fever had ulcers in their legs and feet, which had assumed a scorbutic appearance, from the want of those articles. In patients under these circumstances, the cold affusion was an incalculable blessing. It acted like magic. It was generally used in the evening, and scarcely ever failed to produce a gentle perspiration,

king care of them when ill on ship-board, makes them more attached to the service. Dr. Trotter has very justly marked the loss the service sustained, when the hospital ship was discontinued in the channel-fleet.”

ration, and refreshing sleep, removing the symptoms of fever, and relieving the depression of spirits and anxiety of mind. A saline cathartic, such as I have already mentioned, completed the cure. By these means, we generally got clear of fever while at sea; but it broke out again on our return to Port Royal, because there the men were often on shore, exposed to the heat of the sun, on watering and dock-yard duty; and besides in harbour, discipline could not be so easily preserved, or drunkenness prevented.

“ I was soon so much convinced of the great advantage of using the cold affusion early, that I made a point of seeing immediately any man that complained; and I gave positive directions to my mates, whenever I was out of the ship, to use the same vigilance, and to apply the affusion without delay. And I always kept one of the mates in the sick birth, during the night, to watch the exacerbations of heat, and where they appeared, to take the patient out of bed directly, and pour the salt water over him.

“ At first, my patients were startled with the proposal of this novel remedy; but after a little experience of its effects, they submitted to it cheerfully, and were even anxious to use it. As

soon

soon as the fever went off, I gave the bark infusion or decoction, with a light nutritive diet, and in some cases, a little good wine and porter from the ward-room mess.

“ I leave you to judge what direful effects the lancet must have had, employed on such patients as I have described. In those ships where venesection was used, the mortality was great. But the more general practice, both in ship and on shore, was to trust chiefly to calomel, and to bring on salivation as speedily as possible. Of this medicine, as an auxiliary, I have the highest opinion. I generally used it as a cathartic, as I have already mentioned, and in some cases brought on salivation. But even in these instances, I never hesitated to use the cold affusion on the febrile heat recurring, and I never saw any injury from this practice.

“ I never had recourse to blood-letting in this fever, although I was incited to use it, both by precept and example. I am a decided enemy to this practice in the fevers of the West Indies, and of other warm climates, both from reasoning and observation. It may perhaps be sometimes used with officers just arrived, of full habits, and who live freely. But it will not do with sailors,
and

and especially with those who are impaired by service, or by the climate.*

“ Neither did I encourage the use of emetics, which must ever, I think, be attended with dangerous consequences in this fever, notwithstanding, that in one of the medical journals, a practitioner of Jamaica mentions his giving a solution of the tartarum antimoniale to stop vomiting! In a case on board the Ganges, in

* “ I served in the East Indies five years, (from 1793 to 1798) on board his Majesty’s ship Heroine, commanded by the Hon. Captain Gardner, and the Hon. Captain Murray, and did not use the lancet twelve times in that period of years, except in cases of accident.

“ When the lancet was used, it was chiefly in the incipient state of inflammation of the liver, in new comers. Lord Hobart came home passenger in the Heroine—in our passage from Madras to Spithead, we did not lose a single man, and there was only one man sent to the hospital on our arrival, in the last stage of a liver affection. I contracted a disease of the liver in the East Indies, which appeared again in the West Indies, and obliged me to leave the Ganges, and come home a passenger. Dr. Blane, late Commissioner of sick and hurt, is no stranger to my practice in the East Indies; nor is the ingenious Dr. Clark, of Newcastle, who has made the best practical remarks on the diseases of that climate; remarks which may be depended on.”

which

which one of my mates gave this medicine, it brought on such violent retchings, cold sweats, and languid pulse, as alarmed me greatly. A large dose of tincture of opium, in Madeira wine, stopped these symptoms.

“ The fever which prevailed on board the *anges*, was not confined to the sailors; it extended to the officers. Mr. George Allcot and Mr. William Carter had it. This last had three different attacks of the fever, and every time used the cold affusion. I have not the least doubt that he was saved by it, and that nothing else could have saved him. In the absence of the surgeon of the *St. George*, I was sent for on board that ship, to attend Mr. Yule. As usual, prescribed the cold affusion, and with the usual happy effects. Captain Lobb, who now commands the *Isis*, a man of superior talents in his profession, then commanded the *St. George*. He was much pleased with the practice, and mentioned that he had known one surgeon of the navy before, who had adopted it, and with the greatest success.

“ If you ask whether this was the yellow fever of the West-Indies, I would answer that it was, though under the practice I have described, the yellow tinge of the skin seldom appeared.

By

“ By whatever name it may go, it was no doubt the same fever that spread such destruction through our naval and military service in the West Indies last war. I did not consider it as contagious. There was little chilliness perceptible in this fever—the remissions, if any, were very indistinct. The patients complained of a burning heat, almost from the first.

“ While we lay at Port Royal, a merchantman came down from Kingston, and our purser, Mr. John Allcot, went on board of her to buy some coals. When he returned, he informed me that almost all her men were down in fever, and entreated me to go to their assistance. I found that she had already lost in this fever ten of her seamen and two mates; four men were then dangerously ill of it. They were in a very dirty state; the symptoms were, a hot burning skin, flushed face, red suffusion of the eyes, rapid pulse, anxiety, restlessness, and delirium. I immediately put in practice the cold affusion with them all, and with immediate and striking benefit; I directed it to be repeated from time to time, as the heat might require it. I also sent each of them a dose of calómel and antimonial powder, to be taken next morning. The master of the ship, who had been himself ill of this fever, and with difficulty recovered, was wonderfully

fully struck with this bold practice, and with its obvious good effects. The next day his steward being taken ill of the fever, he himself put him under the cold affusion, and sent to me for the medicine the others had taken. I visited the ship in the evening, and found the steward quite cool and comfortable, and so far recovered from his alarm, which had been great, that he said he would attend his duty next day. All the others were doing well.

“ From the extraordinary success of my practice, I considered it as a duty incumbent on me to publish an account of it, and I accordingly sent one to the Kingston Diary, of July the 25th, 1802, referring to your book, as the authority from which it was derived. Of this I have already sent you a copy. I hope, but am not sure, that it was published, as I sailed immediately after sending it. I also made my success known to all my medical friends. I imagine the practice is scarcely known at Kingston, as a medical man of considerable reputation there, wondered at my temerity, and was astonished at my success. The 2d battalion of the 60th regiment, stationed at Upper Park camp, lost a number of men from fever; they were treated by mercury. On mentioning the success of my practice with the cold affusion to the surgeon, he regretted the

want of a liberal supply of water, to put it in practice, the regiment being supplied by contract from Rock-port, some miles off. On my passage home in his Majesty's ship the Decade, the cold affusion was tried in all the fever cases that occurred, and with similar success to what I have already mentioned.

“ In a short time, it will, I think, have as many advocates as the cow-pox.

“ I am, Sir,

“ Your very obedient and faithful servant,

(Signed)

“ L. F. NAGLE.”

It would be to no purpose to comment on this most important narrative. The documents for the extraordinary exemption from mortality on board the Ganges, are the journals of Mr. Nagle, in the possession of the board of sick and hurt. The means of this exemption are here detailed at large to the world. A comparison of the diseases and deaths on board the Ganges, with those of the other ships on the same station and service during the same period, may be easily made by the board, and the inference, it is presumed, must be irresistible. I shall only add, the water employed for affusion on this occasion, being

being that of the sea, within the tropics, the temperature of which varies from 75° to 79° , it is probable, but not certain, that water of this temperature would be sufficiently cold for the same purpose in any latitude. But to produce the desired effect with water of this heat, more frequent affusions would in all likelihood be required. The only rule observed by Mr. Nagle in this respect, was to repeat the affusions whenever the heat and restlessness of the patient indicated their use.

I might here close the evidence in favour of this practice in fevers on ship-board, but the importance of the subject must plead my apology for offering one other narrative, which differs in some particulars from those already given. The authorities hitherto given, have been countrymen of our own, engaged in service on the seas of the North of Europe, or of the West-Indies; the following statement, is from a foreigner exercising his profession in the Mediterranean sea, or the contiguous parts of the Atlantic ocean. I must preface it by a few remarks.

In the beginning of last March, I received a letter from Dr. Baeta of Lisbon, dated Feb. 2d, 1803, mentioning, that though a stranger, he had thought it incumbent upon him to inform

me of the success which had attended the practice of cold affusion, during the summers of 1801 and 1802, in such cases of the fevers of that city, as it had been employed in; the particulars of which correspond precisely with the general reports on this subject. "In forty cases of fever," says Dr. Baeta, "which fell under my care, the general symptoms were; pulse from 110 to 130, and sometimes even to 140, in the minute; tongue dry and foul; thirst; skin commonly dry; heat of the body generally above 100° of Fahrenheit, sometimes 103° , and in three cases 105° ; high-coloured urine; in some cases petechiæ, also coma, and delirium, with watchfulness and restlessness."

"In the case of a patient, whose symptoms on the 4th day of fever, were; delirium; pulse 130, and small; tongue dry; skin dry; heat 104° ; restlessness and watchfulness; the cold affusion was employed on the 13th July, 1802; one hour after, the pulse was 108, and a little more strong; the heat 99° , with a gentle diaphoresis on the surface; the delirium had disappeared, and the patient felt light and comfortable. On the following day, continuing better, he took some bark and wine, and recovered completely in five days." This may serve as a specimen of Dr. Baeta's success. In communicating

ating it to me, he mentioned, that a friend of his had employed the same remedy on a much larger scale, in a fever on board the Portuguese fleet, the particulars of which he offered to procure me. I accepted this obliging offer, in consequence of which, I received the following communication from Dr. Bernardino Antonio Gomez, physician of the Portuguese fleet, which my limits oblige me in some measure to abridge:

“ *Lisbon, 15th June, 1803.*

“ Sir,

“ As my friend Dr. Baeta informed me, that I had made use of the water of the sea, in the treatment of infectious fever; and as he expressed a desire to know the result, I present you with an account of it, to which indeed you are entitled, since it was from you, through the medium of your estimable work, the *Medical Reports on the effects of Water*, that I learnt the use and efficacy of that remedy; a remedy, which, when the dread which its novelty and singularity excite shall subside, will become as general in practice, as salutary in its effects, and which will render the names of Wright and Bertrami celebrated and revered; the first as its discoverer;

discoverer ; the last as having regulated its application, extended its use, and rendered its utility incontrovertible.*

“ In the beginning of January, 1802, a Portuguese squadron sailed from Lisbon, consisting of three frigates and three brigs, to cruize in the Straits of Gibraltar, and check the incursions of the Algerines into the Atlantic. Two months afterwards it was joined by a ship of the line.

“ From the time of its arrival in the Straits, a fever began to spread throughout the whole squadron, and prevailed to such a degree, that about the middle of March, one of the frigates and a brig returned to Lisbon, with two hundred ill of it on board; and in the beginning of June, when I was sent to the squadron, the sick amounted to two hundred and twenty, not including those in the Swan frigate, taken through

* I feel some difficulty in publishing this high encomium I may plead, that it being shared with Dr. Wright, I have scarcely a right to suppress it; and that it may serve to convey some notion of the impression made by the practice in question, on this accomplished foreigner, to whom Dr. Wright and myself are, personally, equally unknown.

scandalous negligence by the Algerines, in which alone there were forty ill in bed."

Dr. Gomez observes, "that from the universality of this fever on board the Portuguese fleet, in some of the ships scarcely an individual escaping, while several English and American ships of war at Gibraltar, remained in perfect health, there is no doubt that it was contagious, and although he declines tracing the contagion to its source, there is little question that it originated in the want of cleanliness and ventilation, which, without experience, as well as the strictest discipline, are so difficultly preserved on board crowded ships of war.

The fever came on as usual with lassitude and cold rigours, succeeded by severe head-ach, wandering pains over the body, and great heat. The tunica adnata of the eyes was often suffused: in some there was slight ophthalmia, and in others transient symptoms of cynanche. In several instances there were pulmonary affections, constituting what may be called peripneumonia notha; these symptoms of the inflammatory kind did not continue through the disease. There were the other usual symptoms of fever; thirst; nausea; occasional vomiting; white tongue; frequent pulse; the skin dry, and hot to the touch, but in

the few cases in which the heat was measured by Dollond's thermometer, it did not exceed 103° of Fah^t. Hæmorrhage from the nostrils often occurred in the early stages, and when moderate, seemed salutary, by relieving the head-ach; but when profuse was dangerous. The blood drawn in a few cases from the arm, did not exhibit the inflammatory character.

If the fever did not terminate in from four to seven days by a critical sweat, the tongue became dry, black, and tremulous; the pulse feeble, and at times irregular; watchfulness, delirium, and subsultus tendinum came on, with all the usual symptoms of the worst form of typhus. Gangrenous sores appeared on the nates; swellings in the parotid glands, in some during, in others after the fever; and in two instances there appeared buboes in the groin. In short in malignity, this fever seems to have approximated the plague. The favourable termination was either early and sudden by a profuse perspiration, or slow and without apparent crisis. The hæmorrhages, tumours, &c. were never critical.

Dr. Gomez on a review of the symptoms, which he makes at some length, decides, that this was the typhus gravior, or febris sensitiva irritata of Dr. Darwin. During the prevalence
of

these dangerous cases there were milder forms of the disease, in this, as in other respects, the epidemic bearing much resemblance to the plague in the French Egyptian army, as described by Dr. Pugnet*.

“ Although,” says Dr. Gomez, “ the work of yours which I have already quoted, had convinced me of the utility of the cold affusion in typhus, I did not venture to make use of it immediately on my joining the squadron, for the strangeness of the remedy made me more timid than I usually am with regard to new remedies, though of an active kind.

“ I resolved first to become well acquainted with the fever, and to prepare by degrees the minds of my inexperienced assistants and others, to see without murmuring, a practice so singular and unheard of. I began therefore, by the use of water mixed with vinegar, simply as an ablution; I attested the still greater efficacy of the affusion of sea-water; and pointed out in particular the relief which a patient had received in a dangerous state of fever by merely removing to a cooler place.

* *Memoire sur les Fievr. pestil. & insid. du Levant*, p. 125, 214, 215.

“ Having

“ Having by these means made a favourable impression on the bye-standers, I waited only for a proper opportunity to put the affusion in practice. The case of a patient whose pulse was 156, his heat 103°, his skin dry, and mouth parched, seemed to afford this opportunity, and I directed three gallons of sea water to be thrown over him. After drying him and putting him to bed, I found his heat and other symptoms of fever abated, and soon after he went to sleep; but he awoke in a short time, as hot as ever. I therefore ordered him to be put into a tub and washed with sea water till his skin felt cool, but not long enough to give him a painful sensation of cold, or to make him shiver; after this ablution his pulse fell to 136, and the heat of his surface became natural.

“ The next day neither the heat nor the pulse rose so high as before, and as he then complained of pains in his bowels, and of looseness, which last he had had from the first, though it had not been mentioned to me, I did not repeat the affusion or ablution. His feverish symptoms diminished, but the pains in the bowels and the stools increased, and the evacuations at length became bloody, so that it appeared as if a typhus was transferred into a dysentery. The patient was cured, though he suffered a good deal from

the affection of the bowels. In consequence of this case, there might perhaps be added to your contra-indications to the use of cold water in fever, *a tendency to dysentery* *.

“ Although the result of my first trial was not the most fortunate, it proved the power of cold sea water applied externally in diminishing the pulsation of the arteries, the burning heat, and dryness of the mouth; and in procuring sleep; advantages which encouraged me to the free use of the remedy on all proper occasions.

“ This I practised thence-forwards, with such extraordinary success, as produces a sensation in my mind, of regret for not having used it sooner, joined with a singular satisfaction in the consciousness of having been the instrument of bringing into use on board our ships of war, a remedy always at hand, and which possessing almost miraculous virtue against the most frequent and

* The reader will see, that some of my correspondents have used the cold affusion with success, in cases where diarrhœa or dysentery were present; but my own experience rather serves to confirm the observations of Dr. Gomez. Dr. Lind, however, speaks of the cold affusion as a cure for the chronic dysentery of warm climates. *Essay on the health of Seamen*, p. 49.—J. C.

terrible

terrible of diseases that affect navigators, may perhaps be denominated the greatest discovery that has been made in the general practice of medicine." Dr. Gomez adds, that it would be unnecessary and tiresome to detail the vast number of cases in which he made use of the external application of sea-water—*The result was, that he speedily subdued this dreadful fever, and restored the health of the fleet.*

In the application of the remedy, Dr. Gomez employed a method somewhat peculiar. "I have mentioned already," he observes, "that in the first trial I made, I conformed to the method of Dr. Wright, which you, Sir, have recommended and adopted—the method of affusion; but I also added, that I soon after employed in the same case, ablution also; and this last method I afterwards employed in most of the cases that occurred. My practice was to place a tub at the foot of the patient's bed, with a little stool in the middle of it, on which he sat down naked. It was filled about nine or ten inches deep with sea-water; one or two assistants dipped sponges in the water, and washed the patient all over, continuing this practice till the surface became cool to the touch, and the skin began to corrugate, but stopping before the patient began to shiver or horror came on."

Dr. Gomez gives his reasons for using this method, which are shortly these. That he found by experience the cooling effects of a single affusion speedily went off, and that though by repeating the affusion, sufficient refrigeration might in the end be produced, yet that this was not effected without trouble, which the other method saved; while at the same time it was more agreeable, and equally safe and effectual.

“ In your country,” he observes, “ the temperature of sea-water being from 44° to 60° , it may in the rapid method of affusion carry off the excess of heat and even more; but in a place, and at a time, when the heat of the air was from 75° to 79° , and that of the sea water in a bucket from 69° to 72° , the effects of a single affusion speedily went off; still I allow that the affusion when sufficient cold is produced by it, is a method more active and decisive, and on that account preferable in the beginning of fevers, and where the patients are vigorous. But when the progress of the fever has debilitated the patients, and the temperature of the water is much above 60° , I think ablution preferable, being more mild and safe, and equally efficacious.” Dr. Gomez considers the action of the remedy, as producing two different effects, the subtraction of the morbid heat, and the interruption

terruption of the febrile associated actions. The first he justly considers as most important, as indeed absolutely essential to the other, since with the return of the morbid heat the febrile actions must return.

The indications of Dr. Gomez, for the use of the ablution or affusion, are the same as those so often laid down in this volume—morbid heat and dryness of the skin, without topical inflammation or dysentery. “Under these conditions,” says he, “I boldly and happily used this remedy at any period of the fever. Occasionally I examined the heat of the patients by Dollond’s thermometer, and found it from 100° to 103° , but as these examinations exposed me to contagion, through the want of a thermometer properly adapted to the purpose, as they required time, and the number of my patients was very great, I gave up experiments by the thermometer, and trusted to ascertaining the heat by the touch, a method sufficiently exact for directing practice.

“The result of this practice, except in the first trial which I have detailed at length, and in which some inconvenience occurred, was *uniformly advantageous*. By its means I obtained,

“ 1st

" 1st, A reduction of the heat of the skin to its natural temperature.

" 2dly, A grateful sensation all over the frame.

" 3dly, A diminution in the frequency of the pulse, of from 8 to 20 pulsations in the minute.

" 4thly, A diminution of the dryness of the mouth, bad taste, and nausea.

" 5thly, Calm, and refreshing sleep.

" 6thly, A salutary sweat which terminated the fever.

" This sweat appeared in some cases immediately after the patient's removal from the bathing tub into bed; in some it took place during the night; in others the day following.

" In those cases where the fever did not terminate in 24 hours after the bath, the succeeding accessions were always less violent, so that if it was not entirely gone after the second or third bathing, it was so much reduced that I left off the bath, and completed the cure by the cinchona."

Dr. Gomez also notices as a very general effect

effect of the cold bath, the restoration of the tone of the stomach.

To illustrate these general positions, Dr. Gomez details six cases much at large; they are very satisfactory, but so nearly resemble other cases contained in profusion in this volume, that it is unnecessary to insert them. He infers from these cases, and from his general practice, the inutility and occasional disadvantage of emetics, which in some instances were followed by a burning heat not before experienced. He allows however, that they may be useful in cases where the stomach is loaded with bile, but he deprecates their being given in the advanced stages of fever, merely on the ground of nausea being present, a symptom which he considers as arising from debility, and which in this epidemic was very generally removed by the cold ablutions. Neither is he an advocate for blisters. The combination of catarrhal affections, with the contagious fever, did not prevent the use of the cold ablutions or impair their beneficial effects.

Dr. Gomez gave to his patients a nourishing bland diet; and for drink, mucilaginous liquids, and lemonade, with sometimes an eighth part, and in other cases a fourth part of wine. The quantity of wine used on the whole seems to have

recommendation given in vol. i. *p.* 259, to persons engaged in practice in the torrid zone. Continued ablution or immersion may doubtless be employed where necessary, with safety, due regard being always had to the actual effects on the patient's temperature. It is true, in the practice of Mr. Nagle in the West-Indies, which we have just given, affusion seems to have been as effectual as could be desired in subduing morbid heat, though the water of the sea within the tropics is warmer than in the Mediterranean; but some difference will probably be found in this respect, not merely in different cases of fever, but in different epidemics, when the state of the animal heat, the most important of all the symptoms, shall be regularly introduced into the history of febrile diseases.

Here then we close this division of the subject. It must serve to recommend the cold affusion in fever on board of our fleet, that it is not only the best remedy for the sick, but the best means of preventing the progress of infection; with this farther advantage, that it is of all others the most easily applied. If it be compared with the remedies recommended by Dr. Lind, Dr. Blane, and others, its real value will be fairly appreciated.—The science of medicine, hitherto on occasions such as these, comparatively feeble
and

and unavailing, by the adoption of this practice, assumes a higher character, and the sanative powers of nature more than cope in force and rapidity with its destroying powers.

CHAP. V.

Some account of the use of the cold affusion on shore, in the warmer climates.

HAVING in the preceding chapter, stated such information as has reached me, respecting the use of the cold affusion in febrile diseases at sea, I should now give some particulars of its effects in the fevers which have attended our armies on foreign service. My information under this head is however very defective, a circumstance I particularly regret. I have not been favoured with much private information from our military practitioners, and I have to lament, in common with all who are competent to judge on the subject, that the medical history of our military expeditions is not regularly given to the world. This is a duty which might be imposed on the chiefs of the medical staff with propriety and advantage. It would operate as a pledge for their attention;

it

it would bring their knowledge and talents to the tribunal of the public, and particularly of their own profession; and it would be a means of preventing ignorance and presumption from intruding, or being intruded into situations, where their effects must be singularly melancholy and disastrous.

It has already been mentioned, (see vol. i. p. 261) that on the northern shores of the Mediterranean, where the use of the cold bath in fevers was frequent among the ancients; and the use of cold water as a drink, the prevailing practice, these remedies have fallen into disuse in modern times; in part probably from the influence of a false theory, and in part from the sudden and fatal effects which must occasionally have occurred, from the exhibition of such powerful remedies, while the principles which ought to regulate their application were unknown. The salutary practice of antiquity, has been revived in that region under happier auspices, by Dr. Dewar, now physician in Manchester, (1805) assistant surgeon of the second or Queen's regiment of foot, as appears by his letter in the 59th number of *the Medical and Physical Journal*, of which the following is an extract.

“ The Second, or Queen's Regiment of Foot, forming part of Sir Ralph Abercrombie's army,
M 3 arrived.

arrived from England at Minorca, on the 21st of July, 1800. Being sent to attend a detachment of it on board the *Thisbe* frigate, and afterwards doing duty with the whole regiment on shore, I had occasion to observe the facts I am now to describe.

“ The men enjoyed good health while on board, with the exception of two or three cases of intermittent fever, which had broke out on the passage, and a cholera morbus, which appeared in the harbour in an alarming form, though with no fatal consequences. But on the 11th of August, two days after they were landed at Mahon, an ardent fever appeared among them, which in a little time made considerable progress.

“ For a whole week, sixteen men, on an average, were taken ill each day. Their complaints, for the most part, came on suddenly, and very often when they were on parade. After slight languor and debility, the patient was all at once seized with violent head-ach, giddiness, pains, and extreme debility, in the lower extremities, rendering him totally unable either to stand or walk. When he was brought to the hospital, we found him labouring under all the symptoms of the most violent pyrexia, increased heat, quick pulse, and urgent thirst. Two or
 1 three

three of them had very frequently alternations of heat and cold; but, in all the rest, the preternatural heat of the skin was constant, and the patient's feelings uniformly hot and oppressive. The symptom of which they all most violently complained, was the excruciating head-ach.

“ I shall not trouble you with the observations I made respecting the causes of this fever, or the different remedies I employed, but confine myself to an account of the cold affusion. My opinion of its efficacy was previously fixed, from the perusal of Dr. Currie's valuable Reports, and from some striking cases which I had seen in the Edinburgh Infirmary, under the care of Dr. Gregory. I had the happiness to find that Mr. Wells, then surgeon to the regiment, entertained a favourable opinion of this practice, and that we had an opportunity of accommodating a great proportion of the patients in a regimental hospital under our own care. I presaged the happiest consequences from it among the men; and seized with avidity the opportunity offered, not of confirming my own belief in its advantages, but of observing such facts as might further elucidate the subject, and afford additional evidence to convince my medical friends of the high utility of this practice.

was “ In the mode of application, I observed the rules laid down by Dr. Currie, together with such precautions as appeared a priori to be dictated by reason. The patients to whom it was applied, were those whose skin ~~was~~ uniformly hotter than natural, and parched. I never used it where there was much perspiration; but, on the authority of Dr. Currie, I considered a slight moisture of the skin as forming no objection to its use. While the patient laboured in this state under tormenting head-ach, and every symptom of violent fever, I took him out of bed, stripped him quite naked,* and desiring him to hold back his head, and shut his eyes and mouth, poured a quantity of pump water first over the head, then over the breast and back, then washed the arms and palms of the hands, the thighs, legs, and soles of the feet; when the extremities, formerly hot, now became cooler. The heat generally returned about the region of the heart; while, in the head, it continued during the first affusion with little abatement. I therefore again washed the head and breast, and so on alternately, till the whole surface became much cooler than before. After this the patient was laid in bed. If in the course of eight

* “ When I had occasion to apply it to females, their delicacy was saved by allowing them to retain their shift, which was changed immediately after the operation,”

ten minutes, the heat returned with equal or nearly equal intensity, he was taken out, and the operation repeated. The head-ach being the most obstinate symptom, and the last to yield to the cold affusion, the head was ordered to be shaved, and kept constantly cool, with a fold of linen laid lightly on, and dipped in water or sandy. Next day, whatever comparative relief the patient might experience, if any considerable brile heat remained, the operation was repeated. In this application, I conceive it be of great importance to begin with the head. The head-ach is attended with an external heat much greater in the head than over the rest of the body, indicating a peculiar force of increased action in that part of the system. To begin therefore with cooling the head, tends to restore uniformity of action through the system. The same circumstance renders it necessary to keep the head cool during the whole course of the disease.

“ The effect of the cold affusion thus applied, is an immediate relief from the head-ach, from the heat of the skin, and all the symptoms of eximia. In every case the rapidity of the pulse is diminished, and the patient always felt immediate comfort. In many cases, after ten minutes, a gentle perspiration broke out over the body, which still further promoted the cure. Next day,

day, if the febrile symptoms returned, they were always milder, and a second application of the remedy greatly diminished them. The head-ach continued for some time after the other symptoms went off, but by the perpetual use of cooling applications it gradually declined, (seldom requiring the application of a blister) and left the patient with no vestige of fever, except a degree of debility. In most cases a yellow suffusion appeared over the surface in the latter stages of the disease, but it gradually went off, and seemed to require no peculiarity of treatment. As medical men, in adopting a new remedy, are often too zealous for its indiscriminate use, and those authors who treat on the subject might be suspected of overlooking some of the occasional bad consequences of their practice, I made it my business to attend to the varieties of the phenomena, and to observe whether or not in any case, this ^{applic} appellation was ineffectual or seemed hazardous; but I could find no instance of this kind. Those patients who had previously a slight moisture on the skin were benefitted as well as those whose skin was parched. Some shuddered and started when the water was applied, but this unpleasant sensation was very momentary, though I confess, in observing this, I would use the precaution of applying the water rather more gradually, lest an excessive shock should overpower the system in this state of febrile sensibility.

possibility. The only patient in the regiment to whom the fever proved fatal, was an officer, whose obstinate disposition resisted the application of every powerful remedy, and in whom indeed the disease assumed a different type, from his being subject to a constitutional gloominess of mind, increased at that time by misfortune. I attribute the general efficacy of the cold affusion, on this occasion, in a great measure to its early application.

“A similar fever was very prevalent among the natives of this island, but still more so among the British troops, where it broke out in each regiment at a different time. From any information I could collect, the cold affusion seemed to be unknown to the physicians of the island, nor could I even find any instance in which it was employed in our military hospitals. Some of my friends advised me against its use, from the unpleasant speculations to which its novelty might give rise. One gentleman, whom I in vain endeavoured to prevail on to employ it, told me afterwards, that he had, notwithstanding, kept the heads of his patients cool in the same manner as I had done, and found it invariably serviceable for alleviating the head-ach. In many patients in the island the fever was attended with high delirium on the second or third day. None on whom the cold affusion

fusion was used had any delirium worthy of notice. Some of them complained of giddiness in the erect posture, and their minds were observed to waver a little during the night. From this I concluded that a strong tendency to delirium had existed, but was checked by the same remedy which removed the other symptoms. In many the fever was evidently cut short at once, and in all of them I had reason to think that its course was rendered much milder. The emaciation which appeared among the convalescents was not to be compared to that which generally takes place in fevers so violent. The disease, under this treatment, proved much less formidable than in the ships in the harbour, and in other regiments on shore, where its fatality in some instances spread no small consternation.

“Knowing the present spirit of enterprise which prevails in the medical world, I expected on my return to this country in 1802, that the cold affusion must be universally employed, and was rather mortified to find, that though no facts were brought forward to its discredit, many medical men seemed very unwilling to employ it. It is rather singular, that while new articles, formerly reputed poisonous, are daily introduced into the *Materia Medica*, and experiments are made with them not only without scruple, but with

with zeal, the affusion of cold water in fever, a practice frequent among the ancients, and employed with advantage by some rude nations of modern times, should be considered as too extraordinary in its nature to receive a trial."

Edinburgh, Nov. 14, 1803.

It is to be regretted, that the practice of Mr. Dewar was not generally known and imitated in the army of Sir Ralph Abercrombie, a body of men singularly precious to their country.

The use of cold water as a drink in fevers, we know to have been long the practice of the native Egyptians,* and if we may give credit to Savary, the external, as well as the internal use of this remedy is common among them, even in our own days. He observes, in his *Letters on Egypt*, 242, "that if heat were the source of the disorders of that country, the *Said* would be uninhabitable. The burning fever (the causus of the Greeks) is the only one it seems to give rise to, and to which the inhabitants are subject. They soon get rid of it by regimen, drinking a great deal of water, and bathing in the river."

* See Alpinus, lib. ii. cap. 15.

In page 225 of the same work, he gives the case of a master of a ship, a man of credit, as related by himself, who having taken some sailors on board affected by the plague, at Constantino-ple, caught the infection. "I felt," says the master of the ship, "an excessive heat, which
 "made my blood boil, my head was soon at-
 "tacked, and I perceived that I had but a few
 "moments to live. I employed the little judg-
 "ment I had left to make an experiment. I
 "stripped myself quite naked, and laid myself
 "for the remainder of the night on the deck:
 "the copious dew that fell, pierced me to the
 "very bones; in a few hours it rendered my res-
 "piration more free, and my head composed.
 "The agitation of my blood was calmed, and
 "after bathing myself in sea-water, I recovered."

In a former part of this work, (vol. i. p. 396.) I have supposed, that the use of cold water exter-
 nally, which was unknown to the French army in
 Egypt, was equally unknown or neglected in our
 own army there. By a communication from
 James M'Gregor, Esq. superintendant-surgeon to
 that part of our army, which landed in Egypt
 from India, in 1801, I find my supposition was
 not perfectly correct.

"In the pest-houses of the Indian army,
 say

says he, "we were at first very unsuccessful, and trial was given to a variety of modes of practice. I find on an examination of the reports, that bathing with a solution of nitric acid, and sponging the surface with vinegar and water, and lemon-juice and water, were attended with the best effects."

The history that follows supports the narrative of Savary, and very exactly resembles that quoted in vol. i. p. 394, from M. Desgenettes.

"About the middle of October, 1801, a Hindoo, attached to the commissary of cattle, being seized by the plague, was by his surgeon conducted to the pest-house of Rosetta, but on the evening of the same day made his escape, and though fired at by the centinel, got clear off. We heard no more of him for nearly four weeks, when he was found concealed among the reeds on the banks of the Nile, near Boulac, from whence he was sent down to the army then encamped near Rosetta. On his arrival, I inspected him along with his surgeon, Mr. Guild; he was free from bubo and other symptom of fever or plague. He must have lived chiefly on sugarcane, rice, dates, and what he could steal; and being fearful of having incurred military punishment by running away from the pest-house, he

he had concealed himself carefully among the reeds on the banks of the river.

It appears from what Mr. M'Gregor remarks, that the Hindoo could not have made his escape, had he not been in the first stage of the disease, before his strength was broken, for he observes, "that in severe cases of plague, and in the advanced stages, patients were not only incapable of moving themselves, but bore motion of any kind very badly, worse indeed than in any other febrile disease I have witnessed." He adds, "did mental vigour, arising from a determined resolution in the Hindoo to escape from a hospital, *from whence hitherto not one of his countrymen had returned alive, support him in his flight?*" However this may be, from what I myself have seen, I have no doubt that his exposure to the open air by day and night, and perhaps his diet and situation in other respects, conduced to his recovery."

"I find that I was correct in stating to you, that on the continent, in 1794 and 1795, I often saw cases of typhus do well in the waggons, with whom my mode of treatment had failed in our hospitals. The waggons were commonly farmer waggons and carts, which were of course open at the top, and the patients therefore exposed

to the weather. It frequently became my duty to press these waggons, not only for the 88th regiment, but for the sick of the brigade.

“ From my journals while in the West Indies, I could adduce a number of cases where the cold ablution had the very best effects in the yellow fever, and also in the typhus, when it prevailed much in the island of Jersey, in 1794, and 1798, but the practice is now so generally known, that to multiply instances of its success is unnecessary.

“ One other fact appears on my journal while in Egypt, which now strikes me as remarkable, and may not be unworthy of your notice; it ought perhaps to be generally known.

“ After crossing the great desert in July, 1801, from a difficulty in procuring carriage, no ardent spirit was issued to the troops in Upper Egypt. At this time there was much *duty of fatigue*, which for want of followers was done by the soldiers themselves; the other duties were severe upon them; they were frequently exercised, and much in the sun; the heat was excessive; in the soldiers' tents in the middle of the day, the mercury in the thermometer of Fah°. stood at

from 114° to 118°, *but at no time was the Indian army so healthy.*"*

While we are speaking of the countries contiguous to the Mediterranean, it may not be uninteresting to refer to the practice, in fever, in the regions that run southwards, and particularly in Persia during the 17th century, as described by Sir John Chardin.†

The Persian physicians, he informs us, were then, as they are probably still, religiously disciples of Galen, whom they supposed to have been cotemporary with Jesus Christ, and to have had

* These observations are important, and Mr. M'Gregor's authority is of great weight. His field of experience has been uncommonly extensive, and from the time he entered into the army, in 1789, he has kept regular journals of his practice. He has served in all the quarters of the world, particularly in the East and West-Indies, in Egypt, in the campaigns of 1794 and 5, on the continent, in the islands of Guernsey and Jersey, and in Great Britain.

† Sir John Chardin was a French Protestant refugee, by trade a jeweller, in which character he made a number of journeys into Asia. He was knighted by Charles II. His works were published in 10 vols. 12mo. at Amsterdam, in 1688, in French, and it is from this edition I quote. No traveller stands higher for accuracy and fidelity.

much

much intercourse with him. For the dysentery, their most common remedy was sour milk, mixed with rice previously boiled in water till it had become quite dry. Bathing was one of their great remedies, especially in fever, as appears from his own remarkable case, which has often been referred to erroneously, and of which the following is an abridgement.

On the 20th of May, 1674, Chardin had reached Bender-Abassi on the Persian Gulph; opposite to the Isle of Ormus, in the 27th degree of north latitude, where he waited for the arrival of a ship from Surat which was to carry him to India. The place, at this season, was very unhealthy, and being himself and most of his people affected by it, he was advised by the physicians not to remain, but to return through Persia by land. He set out on his return accordingly, and on the 23d was seized with a violent attack of fever with delirium, followed by stupor, from which he recovered with difficulty. He had a French surgeon with him who gave him every assistance in his power. The air of the country where he was taken ill, was so bad, that he resolved to be carried forwards in spite of his weakness; and for this purpose eight men were hired, who made a sort of litter or bier, of canes and branches of trees, on which they undertook to

carry him on their shoulders to the village of Laar. For the two first days of this mode of travelling, the fever continued, with frequent faintings; but on the third day he "had a crisis," and was relieved.

He arrived at Laar, at day-break on the 27th, for they travelled by night only, on account of the excessive heat. There he obtained the assistance of the governor's physician, for the fever had returned with violence. The French surgeon and himself thought the case desperate, but the Persian physician treated it as of no consequence. "You have," said he, gravely, "the fever of Bender, but do not be uneasy, for, with God's blessing I will relieve you from it this very day."—Chardin called out, "I am dying of heat."—"I know it," said he, "but you shall soon be cooled." The physician prescribed a great quantity of medicine, consisting of two emulsions, a cooling confection, and at least a quart of some bitter decoction or infusion, with four bottles of willow-water, and a tea-pot full of ptisan." These arrived by an apothecary about nine o'clock. Chardin swallowed the medicines with extreme difficulty and reluctance, but without apparent benefit. About ten his heat and thirst increased rapidly, and the apothecary told him he should have been happy to have given him snow-water to

to drink, but that snow being scarce, no one could procure any but the governor.

“As in the extreme heat of my fever,” says Chardin, “I thought nothing could be so delicious as drinking snow-water, I sent to beg a little snow of the governor, who sent me some about eleven o’clock; and as I then had the most raging thirst, I drank with more pleasure and avidity than I had ever done in my life. My apothecary was always near me. It was he who administered the liquid to me. He filled a large vase with barley-water and willow-water, put a large lump of snow into it, and when it was half melted, gave me the vessel, and desired me to drink my fill. The pleasure I had in drinking was the greater, because the liquor was very agreeable to the taste, and I took it by the physician’s desire. I was lying on the ground floor of the house, in a cool room, my bed stretched on the ground. Every hour the floor was watered, so that it might be said to have been quite covered with water. But nothing could allay the heat of my malignant fever, which seemed to be irritated rather than abated by so many cooling remedies. My apothecary then ordered my bed to be taken up, saying it heated me; and he spread a thin mat in its place, upon which he made me lie down in my shirt, without any other covering, and then made

two men come and fan me. But this was of no avail, the heat continued as oppressive as ever. The apothecary who paid me the most constant attention, then procured two buckets of cold water, and having placed me on a chair, on which I was supported by two men, poured the water over my body by little and little, from the haunches downwards, and then taking a large bottle of rose-water, bathed, in the same manner, my head, face, arms, and breast. I blessed, in my heart, the Persian practice of medicine, which treated sick persons so voluptuously! But our French surgeon, who was always by me, could not contain his indignation. 'The man is killing you, Sir,' said he to me in a compassionate tone. 'What! *bathe* you with cold water in the heat of a malignant fever, with a pint of emulsion, two pints of decoction, and a pound of confection in your belly, with I do not know how many draughts of snow-water. Depend upon it,' added he, 'that instead of being very soon without fever, as he has promised you, your death will be the end of the business.'—'I do not know what will happen,' answered I, 'but at any rate I do not feel as if I were about to die, as you suppose.' Indeed at that moment I felt the heat within me diminish, and my senses return; upon which, my apothecary having felt my pulse, said, 'Your fever is abating.' It went off from that time so quickly,

quickly, that by one o'clock in the afternoon I was quite free from it, even in the opinion of the French surgeon. He was quite astonished, and I was transported with joy. After having offered up my devotions to God, as to the first cause of my recovery, I said to the apothecary, that to complete my happiness I must see my physician. He will return, said he, by the time the medicines have operated. I took them, as I have said, at nine in the morning, and I had only since that time experienced a kind of heaviness, which swelled me up very much without griping me, so that I imagined they would have no effect upon me, and that their efficacy was exhausted in my continued sweating. But in a quarter of an hour afterwards a looseness seized me, and lasted two whole hours, without any pain or uneasiness. In the evening the physician came to see me, and I received him as a prophet, or as Esculapius himself. He had learned from the apothecary how I had spent the day, and he ordered me a mess of rice boiled in water, with cinnamon, and the bark of dried pomegranate, pounded together. I had taken no nourishment whatever for five days.

“ On the 28th, when I awoke, I was a little feverish, on which account the physician, when he came to see me, ordered me an emulsion of the *cold seeds*, (melon, cucumber, gourds, and pompions)

pions) and a dose of the confection, as the day before, recommending that I should eat raw cucumbers. These remedies were given to me at nine o'clock in the morning, and I did nothing all day but drink, most deliciously, willow-water, and barley-water, cooled with snow, eating raw cucumbers, water-melons, and pears. - Verjuice, in considerable quantity, was put into the mess which I took at noon and in the evening, to give it an agreeable taste, and it most wonderfully lessened my thirst.

“ The next morning, the physician having found me still rather feverish, ordered me medicines similar to those I had taken on the 27th. These purged me during the whole day with so much violence, that I had nearly sunk several times under the effects. The night was still worse than the day, as I passed it in pain, with a violent increase of fever. My physician found me in that state, and, as usual, filled me with consolation ; for, after having felt my pulse attentively, he told me that he was going to give me some draughts that would carry off what fever remained, and deliver me from it entirely. This certainly took place, but I do not know how he accomplished it. I only know that about nine o'clock in the morning I took two pints of emulsion, with a large dose of confection, as on the
 preceding

preceding days, and half an hour afterwards a deep; after which I fell asleep, and when I awoke the afternoon, my head was clear, I was without fever, perfectly tranquil, and, as I thought, entirely restored to health.

“ I was so much transported with joy, that I could not find utterance to my feelings, relying on the word of my physician, whom I thought an oracle, that the fever would return no more.

“ On the morning of the 31st he confirmed his opinion, and ordered me to live ten days together on chicken and rice, without any thing else; at the end of which time he said I might be as usual. I asked him how many days it would be before I could pursue my journey? He told me that two more days of repose would be sufficient, and that I might then set out on horseback. He once more ordered me a great dose of Emissions and cordials as before.

“ On the first of June he came to see me for the last time, saying, that I was no longer in need of his visits; that he had ordered the apothecary to bring me the materials for emulsions, and to teach my servant how to prepare them; also, a box of salts, and thirty-five drachms of cooling confection, of which

which I was to take one drachm daily, when I awoke, and to drink after it a glass of water. He said it was to warm and fortify my stomach, which so many emulsions and cooling medicines had considerably weakened."

On the 3d of June, Sir John Chardin proceeded on his journey, and experienced no relapse; and on the 17th he reached Chiraz, in a convalescent state and still weak, but otherwise so well that he left off his medicines.*

The fever of Sir John Chardin was, no doubt, the bilious remittent fever, so common in the East-Indies, in various parts of Africa and America, and indeed in every part of the earth, where the heat is great, and the soil low and moist, as on the shores of the Persian Gulph. It is worthy of remark, that he improved, while travelling exposed to the open air and the dews of night: his case, in this instance, corroborating the facts mentioned in pages 11, 12 of this volume, and the reasonings by which they are accounted for.

The manner of applying cold in this case corresponds with the principles I have laid down. The Persian physician attacked the fever at the

* Voyages de M. le Chevalier Chardin, tom. ix. p. 298.

weight of the exacerbation; he had in view the subduing of the morbid heat, and persevered till this was effected. The laxative medicines were no doubt of service in carrying off the morbid contents of the alimentary canal; but they seem to have been carried to excess on the 29th of May, and to this circumstance the return of fever may be imputed. The bitter medicine was no doubt a decoction or infusion of some vegetable, in its qualities perhaps resembling the cinchona; and the cooling confection was certainly the celebrated mithridate, a combination of opium and aromatics, well known over the eastern as well as the western world.

Whether the same treatment of fever as that described by Chardin, has continued in Persia down to the present times, I am not informed. That a similar treatment prevails in the corresponding climate of Africa, appears from the information of Bruce. "Masuah," says Bruce, "is very unwholesome, as indeed is the whole coast of the Red Sea, from Suez to Babelmandel, but more especially between the tropics. Violent fevers, called there *Nedad*, are very prevalent, and generally terminate on the third day in death. If the patient survives till the fifth day, he very often recovers, by drinking water only, and throwing a quantity of cold water over him, even
in

in his bed, where he is permitted to lie without attempting to make him dry, till another deluge adds to the first."* The expression used here would lead us to suppose, that the external use of cold water is not resorted to unless the patient survives to the fifth day. If this be the case, some miserable theory, such as the doctrine of concoction, or of lentor in the blood, has probably penetrated among these poor Africans, to persuade them that this remedy is not to be used in the early stages of fever, thus limiting and almost destroying its efficacy, and counteracting the evident indications of nature.

Bruce describes the same fever as prevailing in Abyssinia, especially in all low, marshy grounds. "It is really," he says, "a malignant tertian. It always begins with a shivering and head-ach, a heavy eye, and an inclination to vomit. The face assumes a remarkable yellow appearance." This is doubtless the yellow fever of the West-Indies and America. In Abyssinia as well as Nubia, the internal and external use of cold water seems to be resorted to freely in this fever, and with happy effects. When we speak of the cold water of those *uniformly* sultry regions of the world, we ought not to forget, that the

* Bruce's Travels, vol. iii. p. 35.

water even of their springs, being never much colder than the air, is, in general, equal in heat to that of the Buxton bath, and rises of course to what we denominate *tepid* in this climate. At Masuah, Bruce found the mercury in Fahrenheit's thermometer to range from 83° to 92°, which gives a still higher medium temperature, (the probable temperature of the springs) and hence he seems justified in his advice to the traveller of Nubia, never to scruple throwing himself into the coldest spring he can find, whatever may be his heat. It is owing to this high temperature of the water, that fatal accidents, from the improper use of cold drink or the cold bath, have seldom or never occurred in those countries, and that the external and internal use of water in fever has continued from age to age.

The success of the external application of cold water in fevers, in the neighbouring countries, and in similar climates, would lead us to expect a corresponding success in the fevers of India. It is not, however, to be concealed, that Mr. M'Gregor, whose communication respecting the diseases in Egypt, I have quoted so largely, does not confirm this expectation. In the concluding part of that communication he observes, "in the fever most common in India, the cold bath did not

“ not succeed with me. On my arrival there I
 “ tried it in several cases, but it failed. This fever
 “ is commonly of the remittent type; there is
 “ much reaction; it seems in most cases to be
 “ symptomatic of liver affection, and often termi-
 “ nates in hepatitis.” Where fever originates in
 congestion of the liver, or any of the other viscera,
 as no doubt it often does both in the East and West-
 Indies, its cure cannot be expected from the cold
 affusion, which may not, even in such cases, be
 safe. But with every respect for Mr. M’Gregor,
 I can scarcely doubt, that in the idiopathic fevers
 of the East-Indies it will be equally successful as
 in other regions, due regard being always paid in
 administering it to the stage of the fever and the
 temperature of the patient.

“ I remember,” says Dr. M’Lean*, “ when in
 the East-Indies, on board the Airly Castle Indiaman,
 some cases of remittent fever occurred at Diamond
 harbour, where the company’s ships anchor. Several
 perished in spite of every attention. One of the
 patients, however, in a fit of delirium, jumped out
 of one of the ports. He was immediately picked
 up, rubbed dry, and put to bed. His senses re-
 turned instantly; his pulse became more regular;

* Enquiry into the nature and causes of the great mor-
 tality among the troops at St. Domingo, by Hector M’Lean,
 M. D. p. 148.

fell into a profound sleep, and next morning there was a complete remission. He recovered afterwards very speedily.

“ I recollect another instance perfectly similar, on board the Princess Amelia East-Indiaman, at the same place. *The remittent fever had carried off more than half the ship's company*, though every assistance, every comfort the sick could have, was procured by Captain Millet, the humane commander of the ship. A seaman of the name Davies, a very stout, athletic man, in whom the remittent fever had at times alternated with epilepsy, jumped overboard; at the moment he fell this an alligator was along-side the ship. He seemed to become at once sensible of his danger*, and swam with great vigour till he was assisted. I saw him the moment he came on deck; his countenance, which was before grim and unprosperous, assumed a more mild and temperate aspect; his pulse, which had been extremely quick and feeble, was now slower and fuller; and his collection, which had been confused and indistinct, became clear and accurate. I directed him to be washed over with brandy and put to bed. He fell into a profound sleep, which terminated in a universal perspiration, warm and profuse. The

* The plunge had restored his intellect. J. C.

consequences were a very distinct remission and speedy recovery.

“ These cases made a strong impression on my mind, and I was determined to take an opportunity of imitating a practice which accident had pointed out. I had not then seen the book of my friend Dr. Jackson. An opportunity was soon furnished. Upwards of thirty men were in my ward at the Diamond harbour hospital, and I commenced dashing buckets of water over them from some height; but whether the water was not sufficiently cool, or the patient being in expectation, did not feel the shock, or that the circumstances were really different, I do not know, but I was by no means so successful as I had hoped from the two cases I have just related.”

The two first cases sufficiently demonstrate the power of the cold-bath in the fever of the East-Indies, when properly applied. The patients did not theorize on the subject, but obeying the instinct of nature, jumped into the sea in the moment of delirium, when the heat and thirst were at the height.* Supposing the principles which I have laid down for the proper administration of this

* See p. 115 of this volume.

remedy to be well-founded, the candour of Dr. M'Lean has enabled us to explain why he was comparatively unsuccessful in his use of it on shore. The thirty cases in the hospital must have been in different stages of their progress, but he seems to have employed it in them all indiscriminately, and on a great part of course improperly; or, if he selected his cases (which there is no reason to suppose) his principle of selection we may safely infer was a bad one, since the following are the directions he lays down for the use of this remedy a few sentences afterwards. "In the very early stage of fever, before it has established its peculiar mode of action; *before the re-action begins*, I think the practice of dashing cold water on the patient may be useful. But after the fever has established its peculiar mode of action; *after the circulation and vessels re-act*, after determinations to particular organs have begun, I hold this practice less certain." There is another stage of fever in which Dr. M'Lean thinks it may be used. "In cases where the sensibility is much impaired, where the recollection is confused, where the system is as it were oppressed, and wants energy to remove the oppression, *where the pulse is feeble and frequent*, in such cases, I hold the dashing of cold water to be one of the best and most powerful remedies." Again, "I am of

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opinion with Dr. Rush, that it will be most useful where there is greatest diminution of the nervous energy." The language of Dr. M'Lean is not very precise, and the directions are not perhaps perfectly consistent with each other. But it would seem, that he prefers the use of the cold dash in the first or cold stage of fever, before the reaction and febrile heat begin; or in the latter stage, when the vigour is decayed, and the heat sinking the precise situations in which I deprecate its use. I cannot, therefore, be surprised at the following declaration, which certainly reflects honour on his candour: "We have much to learn from experience on this subject. It must not be concealed, that I have employed it often without success, in cases where I promised myself much from its use." Like his friend Dr. Jackson, Dr. M'Lean ascribes the beneficial influence of this remedy wholly to the effects produced through the medium of sensation, and overlooks the changes which it occasions on the living temperature by which the febrile motions are so essentially effected.

Dr. Jackson, like Dr. M'Lean, was led to employ the cold bath in fever, by hearing of the sudden and extraordinary recovery which had occurred in the case of some seamen, who in the delirium of fever, had thrown themselves

into

into the sea* at the Havannah. This, he informs us, was communicated to him while he resided in Jamaica, so early as 1774. It does not appear however that he adopted fully this bold practice, either in form or substance. He used the method of affusion, but as it should seem partially, on the head and shoulders only. The effects were salutary; but he observes, that he did not carry the practice so far, that the fever could in any instance be said "to be precipitately extinguished by it," which is indeed a sufficient proof of the limited manner in which the remedy must have been employed.

In 1798, a year after the first edition of this publication, Dr. Jackson gave to the world, his *Outline of the history and cure of fevers*. In the interval between this and his former work, his experience of the remedy in question had extended, and his stile and mode of reasoning had undergone a remarkable alteration. I do not profess always to understand the full import of the new phraseology of Dr. Jackson, which is in a considerable degree founded on opinions peculiar to himself; nor do I, after a diligent comparison of his precepts with his prac-

* A treatise on the fevers of Jamaica, notes at the end of 1774, by R. Jackson, M. D. 1791.

tice, perceive clearly the principles which regulated him in the use of this powerful remedy. I can however clearly see, that there is little or nothing in common between us in this respect. Instead of employing the cold bath in the first stages, when the strength is nearly entire, and the febrile heat at its height, he premised copious bleedings and other evacuants, by which both must have been previously reduced. Instead of considering the heat of the patient, as the principal circumstance to be regarded in regulating the use of this remedy, he appears to have neglected all considerations of temperature in applying it, except in so far as temperature is connected with sensation; and he has not given us a single thermometrical observation in the whole details of his practice, either in regard to the water employed, the previous heat of the patient, or the change produced by the affusion. The previous or subsequent dryness or moisture of the skin, forms no part of his consideration, and even the very remarkable and almost uniform effect of this remedy, on the pulse, is entirely unnoted.

Nor have I the satisfaction of agreeing with Dr. Jackson, on the manner in which the sensibility of the patient ought to influence us in the use of the cold affusion. He requires a high state of excitement, or of sensibility, on the surface

in the application of this remedy, and considers its benefit as wholly dependant on this previous condition; whereas an extreme sensibility to impressions of cold, (which according to my experience, often attends general sensibility of the surface in fevers) deterred me from employing it, even when the actual heat of the patient indicated its use.

It would not become me to pronounce judgment on the very important differences between Dr. Jackson and myself; this must be left to future observers. But from the experience which I have detailed, I cannot be surprised at the imperfect success of the remedy in his hands, or that other practitioners in the West Indies, who had adopted it on the same or similar principles, should have afterwards abandoned it.*

Of

* In his Remarks on the constitution of the medical department of the British army, published in 1803, Dr. Jackson has again entered on the treatment of fever, and on the use of bathing, as a remedy, at considerable length. On this occasion he mentions my name, as having, by "the popular manner in which I have treated the subject," drawn some notice to the remedy; but claims to himself the merit of having used it for 30 years, and of having communicated it to the world, as it would appear, the first of our countrymen;

Of this number, was my friend Dr. Ord, of Demerara. I had seen this gentleman in Liverpool, in 1795, then on his way to the West Indies, and had mentioned to him generally, but not, it should

in 1791. Here, as in his two former publications, Dr. Jackson unaccountably neglects to introduce the name of Dr. Wright, who, from MSS. in my possession, unquestionably used the cold affusion, in small pox, several years before 1774, and who communicated, for publication, his remarkable narrative, with which this work commences, to the Medical Society in London, in 1779. This most important narrative, in consequence of accidental circumstances, was not inserted in the only volume of the Medical Observations and Inquiries, afterwards published by that society, but was recovered from among their papers in 1786, on Dr. Wright's return to Europe, and given to the world in the widely circulating journal of Dr. Simmons, in that year; *five years, of course previous to the first publication of Dr. Jackson.*

In the Remarks above referred to, Dr. Jackson defends his doctrines respecting the use of cold-bathing, in fevers, with some seeming modifications. He now considers "the presence of heat (i. e. morbid heat) in the patient, however produced, as a general index of forming a judgment of the result," but this obscure expression does not mean, that when present it indicates the use of the remedy, or that its absence prohibits it.

Dr. Jackson occupies several pages in controverting the rules I have laid down for the application of cold to the surface, and in laying down others of his own. Experience must decide between us, and to that I appeal.

should seem, with sufficient precision as to the mode of applying it, the great success of the cold bath in fever. Dr. Ord observes in a letter to me, that he did not conceive the principle on which I used it, was to procure the solution of a febrile paroxysm, but to assist with other tonics in restoring strength to the constitution, after the febrile catenation had been removed. "After the embarkation of the troops," says Dr. Ord, "from that fatal island (Spike Island, near Cork) where so many brave fellows suffered from the inclemency of the weather, and from which were brought the seeds of disease, that carried thousands more to an untimely grave in the West Indies, I made frequent trials of the cold bath, on the principles already mentioned, but little good effects were discernable. Again in this colony (Demerary) when a dreadful fever baffled every other remedy, I made ample trials, with as little effect. A stop was given to every farther trial, by the effects of the cold affusion, on myself in December, 1796. I was attacked by a double tertian, which having long resisted the free use of the bark, I

If I were inclined to enter into any thing like controversy with him, I should soon find myself involved in that obscurity with which his peculiar phraseology invests every medical subject on which he treats.

employed the cold affusion very liberally. Syncope was induced, and on my recovery by means of volatiles and frictions, I felt an intense burning heat, which was succeeded by an infinite number of white patches, exhibiting the appearance of urticaria. Great languor and debility supervened and no sweating stage followed. I recovered from the disease at length by means of a sea-voyage.

“ From the perusal of your valuable publication, which the honourable K. H. Mackenzie put into my hands, soon after my recovery, I became sensible, that I had misunderstood the laws by which the affusion ought to have been regulated, and it proves to me (a melancholy reflection) that I have too often seen the fatal effects of its improper application. I have since trod on sure ground, and to a late use of the remedy in my own case, in another attack of fever, I ascribe my preservation.

“ Towards the end of July, (1799) being exposed to much fatigue, and also to contagion, I suffered a very severe attack of fever, attended with some unfavourable symptoms ; as great irritability of stomach from the first attack ; violent head-ach, with inflamed eyes ; excessive restlessness ; much general yellowness, with vibices on the breast ; and on the fifth day, an almost total deprivation of consciousness. Blood-letting, mercurials,

rials, antimonial, blisters, and bark, both by the mouth and by clyster, had been employed with little effect. On the morning of the 6th day, I was sensible, and conceived my death fast approaching. The heat of my body was insupportable, and I made myself so far understood by my attendants, as to request to be bathed, I was placed in a tub, and four gallons of cold rain water taken out of porous earthen jars, of the temperature of 74° (as I have since ascertained) were thrown over my head and shoulders. The most grateful change was produced by the shock. I was moved into bed, and slept about half an hour. Again the heat of my hands and feet distressing me, the affusion was repeated with the most marked success. I recovered my consciousness perfectly, drank half a pint of claret, and was asleep. I will not tire you with useless repetitions; I resorted to the affusion as often as the heat returned, and with similar effects; I was soon convalescent, and recovered favourably.

I am fully persuaded that the cold affusion is one of the happiest remedies ever recommended for fever, at the same time that it is highly grateful to the patient; nor so far as I know, does it stand in the way of any other remedy; for under the same observation I employed it in my own person without detriment. I know indeed that it calls into

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action

action the mercurial disease, and that it may be employed with safety, as an auxiliary to that invaluable remedy, mercury."

The letter of this amiable and accomplished physician, from whom I quote, dated Jan. 1800, contains a variety of other important observations, and was accompanied by several other cases in which the cold affusion had been used in fever, with the same happy effects. In these all the particulars requisite for accurate judgment are detailed with the greatest perspicuity, and I am only prevented from inserting them here, by the length to which this work already extends, and the necessity of hastening to a conclusion. The result of the whole is, that in Demerary as in the other sultry climates of the earth, the cold bath is a most valuable remedy in fevers: that in cases a few days advanced, it requires to be again and again employed, and may be so employed with perfect safety; and that, as in the fevers of such countries, visceral obstructions often attend fevers, sometimes in the relation of cause, sometimes of effect, mercury is frequently required in combination with the cold bath, with which it may be conjoined, contrary to all former theories, but in correspondence to the facts mentioned in the first edition of this work, not merely with impunity but with the utmost advantage.

These conclusions perfectly accord with the opinions of Dr. Chisholm, expressed in the second edition of his *Essay on the malignant Pestilential Fever of the West Indies* *, already quoted in this work, vol. i. p. 279 and 295.

Dr. Chisholm has expressed himself on the use of cold bathing in fever with candour and liberality.

“ When I first began the use of cold bathing in these fevers,” says he, “ I conceived that the only periods at which it exhibited its beneficial powers, were the commencement, or rather before the fever is formed, and the low state, when the mercury has had no effect, and when the gangrenous disposition has begun its approaches. While this was my idea, I confess I could not satisfy myself with any reasons why its operative effect should be confined to these periods. I could readily account, indeed, for the advantages derived

* See Dr. Chisholm's *Essay*, second edition, vol. i. p. 457.

See also the whole of his appendix, vol. ii. p. 461. The safety and efficacy of the combination of mercury and the cold bath in fever, are confirmed by the experience of Dr. Chisholm, and Dr. Armstrong, of St. Kitts. For an account of Dr. Armstrong's practice, see vol. i. p. 388-9 of Dr. Chisholm's *Essay*, &c,

from

from its early exhibition (i. e. before the fever is fairly formed) by supposing the excitement of a new action sufficient to overpower the morbid action before its establishment; and the stimulant powers of cold clearly elucidated the benefits arising from the application of it in the low state. But has not cold a sedative power also? And if it has, why may it not be applied to diminish morbid heat during the first stage, or the inflammatory state? Authority, not experience, led me to avoid the trial; and such reasoning as the following confirmed my apprehension of danger from it. If we suppose that the operative effect of cold is the production of a new action, which in one instance prevents, and in the other resists, that of the remote cause, how are we to expect, from it the removal of an inflammatory diathesis, which stimuli have already excited? This is specious, but like most reasoning founded on theory, or badly ascertained fact, it is also fallacious. Dr. Currie, of Liverpool, first drew the veil aside, and exhibited the nature and cause of the fallacy. Defective observation and total ignorance relative to animal heat, in a healthy, and in a morbid state, I was convinced with him, were the true causes that cold bathing had hitherto produced but very partial benefit in the West Indies, during the inflammatory stage of fever in general." Dr. Chisholm regrets that this con-

viction

fiction did not come earlier for the regulation of his practice. His experience of the effects of cold bathing, regulated by the principles which I have laid down, he gives in the 7th appendix to his second volume, to which I have already referred. The observations of Dr. Chisholm, were much extended in conversation, when I had the pleasure of meeting him at the house of a friend in the neighbourhood of Liverpool, in the summer of 1803, and of finding, that most of the opinions I had given, as well in regard to the prevention, as the cure of diseases in the warm climates, were remarkably confirmed by the ample experience of this able and accurate observer.

A similar satisfaction I enjoyed in Liverpool, in December, 1802, in the conversation of Dr. Macneil, Inspector-General of the hospitals in Guiana. This gentleman communicated the result of his practice with the cold affusion at Surinam, in 1801; particularly in two cases, those of Mr. Mackenzie, paymaster of the 39th regiment, and of Captain Cameron of the same corps, in which he employed the remedy to a great extent, and with a happy issue. The particulars of these cases I took down in writing, and would certainly have introduced at large here, but for the reasons which have obliged me to omit many
other

other valuable communications. An abridged account I will however give.

Mr. Mackenzie was in the 4th day of fever when Dr. Macneil first saw him, his bowels constipated, his stomach rejecting every thing, and his strength sinking fast. His bowels were opened with calomel, and his stomach composed by laudanum and æther, which also procured him sleep. On the 5th morning he was more composed, and for the convenience of attendance he was carried by water in a covered boat, five or six miles, by which excursion he seemed rather benefitted than injured. He now took the bark, with saline effervescing draughts, and a draught of laudanum and æther at night. On the 6th morning he was completely restless and delirious, his skin dry and burning, with great thirst, and sense of heat. He continued his saline draughts and lemonade as drink, but threw them up, and at his own instance he drank largely of cold water, that stood by him, which seeming to agree with him, his whole surface was spunged with it and afterwards dried. By these means his heat was much reduced, and his mind composed. He was afterwards, at his own request, frequently spunged all over with cold water during the day and night, and he would not allow himself to be dried, but suffered the moisture

to

to evaporate, which he said was in a high degree pleasant and cooling. In this wet state he would not allow even the sheet to lie upon him, but stripped off his shirt and lay quite naked, that he might enjoy the breeze from the open windows, which blew over him. The heat of the day might at this time be from 84° to 88° . In this way the fever was reduced, the delirium and agitation carried off, and the mind rendered calm and composed. His strength was sustained by bark and nutritious diet, his bowels were kept open by calomel and clysters, and æther and laudanum given to render his nights tranquil. As often as the febrile heat returned, cold water was employed internally and externally, as already described, and always with similar effects. In eight or ten days the disease was removed.

The case of Captain Cameron, is still more remarkable. His fever was again and again removed by the use of the cold drink, and of the cold affusion, which he carried to the length of being drenched in his wet clothes. But as he refused all medicine and cordial nutriment, the fever returned, in part probably from his continued exposure to the causes which originally produced it. After each succeeding paroxysm his strength diminished sensibly. At length the intermissions became imperfect, delirium occurred, and the most desperate symptoms supervened. He was finally
saved

saved by the use of mercurial inunction, æther, laudanum, bark, and wine.

“ I think,” said Dr. Macneil, “ the cold affusion an admirable remedy for subduing the paroxysm of fever, and my experience of the time of using it, as well as of the other cautions, agrees with the rules laid down by you in the *Medical Reports*. But it is necessary to follow up the advantage you obtain, with bark and nourishing diet, otherwise the paroxysm is apt to return, and though subdued again and again, to return as before. I also incline to think, that in many cases the wetting of the whole surface, and allowing it to dry by evaporation, is less fatiguing and more safe, than the cold dash, especially when the patient has been weakened by the climate, or the progress of the disease.”

“ I incline to think that the fevers of Guiana are contagious, though the constant ventilation of the habitations, which is enforced by the climate, seldom renders this very striking or obvious. Ablutions of the surface with water will no doubt render the operation of contagion still less.

“ The fevers of Guiana are not so violent as those of the windward islands, and do not run their
course

course so rapidly: the remissions in the first days are more distinct. But if these are not taken advantage of, the fever becomes continued, and terminates generally in eight, nine, or ten days. After apparent convalescence, returns are frequent, unless bark have been used. I have employed the warm bath by immersion, with our soldiers, in the stages when the cold affusion could not be employed. It relieved head-ach, and assisted in opening the skin, but had not the permanent or powerful effects of the cold affusion.

“ Against the irritability of the stomach, the best preventative was keeping the bowels open. I wished my patients to have two stools at least a day, and even more seemed of use in some cases. Calomel given in quantity very frequently brought on vomiting, and I did not depend on it as a cathartic without its being combined with jalap, or the cathartic extract. Where I wished for the specific effect of mercury, I preferred employing the ointment by inunction.

“ Physicians in the West Indies, have gone from one extreme to another in regard to the bark. Formerly they began with it immediately, without waiting for remissions. And this not succeeding, they often abandoned it entirely and flew to other means. I was attacked with fever myself, but a

smart cathartic produced a remission, when I threw in the bark largely, which prevented a return. To me it seems an admirable remedy, but it must not be used till the morbid heat is subdued, and the skin opened. Cathartics and the cold affusion produce these happy effects, and then the bark should be thrown in as fast as the stomach will bear it.

“ The same caution should be used as to wine. It should not be employed in the first stages when the febrile heat is great, and the skin dry, but after the heat is in some degree subdued, and the skin opened, it is of great advantage.

“ Neither do I ever give opium in the first stages of fever. It is of the utmost consequence to keep the bowels free, to lessen the heat, and to open the skin, but these objects obtained and ensured, opium is of great use in allaying irritation and procuring sleep. It may also be used with advantage in the paroxysm of fever when the hot stage is declining, and the sweating stage has commenced.

“ I never found it necessary to give more than an ounce of the bark in powder, or even boiled into a decoction, in the twenty-four hours. I generally combined it with rhubarb in the early stages of its use, unless the bowels were open by other

other *means*. I was always desirous that my patients should have several stools during the first stages of taking the bark.

“ In Guiana, fevers are most severe on Europeans newly arrived. They are brought on by intemperance, by fatigue under the heat of the sun, and by exposure to the rain, or evening dews, after the body has sustained the burning heat of the day. In short, all debilitating causes predispose to them. The country is low and swampy, the dews of night are very heavy, the exhalations from the rivers, canals, and ditches, very considerable, and loaded with miasmata. Fevers there, generally begin with a rigor, but not always; head-ach, great heat, dry skin, pains across the thighs and the small of the back, and in some cases cramps in the calves of the legs, may be considered as the general symptoms.

“ The water used in Surinam, is excellent. It is rain water collected in cisterns under ground, and suffered to depurate. It is afterwards passed through a filtering stone, and kept in evaporating earthen jars, there manufactured by the Indians, in which it acquires a temperature from eight to ten degrees lower than the atmosphere*.

* These observations I took down from Dr. Macneill, in conversation. They were afterwards revised by him. J. C.

These observations of Dr. Macneill on the proper remedies in fevers, accord entirely with my opinions, and I earnestly recommend them to the attention of medical practitioners, in warm climates, and particularly in the torrid zone.

It was also my good fortune to become personally acquainted with Dr. James Robertson, surgeon to the naval hospital at Barbadoes, when on a visit in Liverpool, in the winter of 1799—1800, and to receive several valuable communications from him, after his return to the West Indies, one of which is inserted at large in vol. i. *p.* 292, of this work. A letter from him, dated the 16th of March, 1801, contains a valuable dissertation on the fevers of the West Indies, which I regret that I have it not in my power, for the reason already repeatedly mentioned to give at large. I cannot however refrain from making extracts of such parts of it as more immediately respect that mode of treatment of fever which it is the principal object of this publication to explain and to enforce.

“Till I read your book, and learned the result of your experiments, and the principles on which the effects of the cold affusion were explained, I never had had recourse to it. For soon after

I came

I came to this island (Barbadoes) in 1793 or 1794, a gentleman lately from Europe had the cold affusion tried upon him *after all other expedients had been resorted to in vain*; but he died during the operation, or shortly after it. I did not see the case, but have no doubt that the powers of life were too much reduced at the time the remedy was tried. The catastrophe made some noise at the time, and put a stop to the practice. It however had been previously resorted to at different times by our practitioners, or others, but always I believe as a last resort. In one case a gentleman of the name of M——e, ill of fever, was despaired of by his physicians. He was yellow all over, and had been delirious for some days. He had incessant vomiting, with hiccup, and petechiæ had spread over his body. In short, his dissolution seemed fast approaching, when General P——, determined to try the following experiment. The sick man was stripped naked, and fresh lime juice poured copiously over his body, frictions being at the same time employed. He was afterwards dried and put to bed. A general sweat broke out, followed by refreshing sleep. He recovered, and is alive at this day. I have not been able to learn the state of the surface as to heat at the time this remedy was tried.” Dr. Robertson observes, that in the use of the cold affusion he has conformed to the directions I have

laid down, of which from experience he gives his decided approbation. "I am frequently called on board merchant-ships," says he, "to prescribe for men who are ill of fever, and as it is often inconvenient to send them on shore, I have frequently succeeded in extinguishing fevers in the first instance, by the affusion of cold sea-water on board. The patients are well dried and rubbed after the affusion, and lodged in a dry part of the ship. Two or three pills of calomel, of two grains each, are given at intervals of an hour, and an opiate, with some mild diaphoretic, is exhibited at bed-time. The cold affusion is directed to be repeated, whenever they feel the sensation of burning heat return, with a dry skin, of the application of which simple rule the master or mate is always a competent judge.

"When patients are received at the naval hospital from ships of war, they are generally bad cases, whatever be the nature of the disease, as by the rules of the service, none are to be sent to the hospital who can be cured on board. Hence it does not often happen, that I receive cases of fever from our ships of war in an early stage of the disease, and the fever-patients sent to the hospital often labour under other affections, which forbid the use of the cold affusion. But every patient in fever, as soon as received, is spunged with vinegar
and

and water all over, before he is put to bed, if he feels hot, and the cold affusion is had recourse to afterwards if the case seems to require it.

“ With prisoners of war the case is different. The sick of this description are under my care from the first, and as soon as any in jail appear to be in a feverish state, they are sent to the hospital. A part of the discipline of the hospital has all along been to wash the patients on their first admission, before they put on their hospital dress. And sometimes I have sent men back to the jail in a few days, without administering any other remedy than this ablution; the change of air, diet, and lodging, no doubt materially tending to arrest the progress of the disease.

“ The cold water used by me in affusion has seldom been below that degree of temperature to which you give the appellation of *cool* (74° or 75° of Fah^t.) and I am apt to believe, that that degree of temperature in this country is equally efficacious with a much lower degree in a cold country; for the sensations of the body are more acute, the fibre is more irritable in this country than in Europe, of which fact I have very recent experience in my own person,

“ The *cool* affusion, to adopt your relative
P 4 term,

term, may be considered as *cold* in this country, and I consider it to be not only adequate to the desired effects, but more applicable to cases of fever here, at least among natives, and long resident inhabitants, than the absolutely cold affusion; though in certain cases, especially those of new settlers from Europe, this last might perhaps be preferable. I have however found, so far as my experience extends, the medium temperature of our seas and springs sufficient for every purpose*.

“ And in cases where, from local affections or other circumstances, the cold affusion is not judged expedient, milder modes of the application of cold have been adopted. Such as the sponging with water and vinegar; the *lavatio frigida* of Dr. Wright; frequent bathing of the hands and face in cold water; dipping the face into cold water; chafing the wrists, hands, neck, and temples, with camphorated vinegar or spirits, and the application

* “ The temperature of the sea-water here (Barbadoes) at and near the shore, is about 77° or 78° in the heat of the day. But if the water be contained in a receptacle, and exposed to the sun, the temperature is sometimes higher. About sun-rise, the coolest time of the day, the sea on the shore is about 72°, which is nearly the heat of deep wells and springs in shady situations.”

the cold fresh leaves of the sand-box tree, or
 phalic vine, about the temples, and rubbing the
 ns with lemons, or limes, cut transversely.

“ Indeed the salutary effect of calomel, and
 mes s powder, where the latter is admissible (the
 te of the stomach, in this country, frequently
 prohibiting the use of any preparation of anti-
 ny) may in a great measure be referred to their
 oling effects; for these medicines, by opening
 bowels, and increasing perspiration, must tend
 prevent the accumulation of morbid heat, as
 ll as by removing congestions, on which the
 rile heat depends. And I have never found it
 edient to continue the use of either calomel or
 imonials, in fevers, after the skin becomes soft,
 tongue moist, and the belly open, unless with
 view of preventing, or removing, topical affec-
 as of the viscera, which I am confident that
 k, either too early or too copiously administered,
 pt to give rise to, or to confirm.”

To Dr. Robertson I am also indebted for some
 communications, made to him by Dr. Davidson,
 St. Vincent's, of which the following is an
 extract :

In 1791, and 1792,” says Dr. Robertson,

“ we

" we had at St. Vincent's, a low nervous fever, which evidently seemed to be the offspring of contagion. It began in the small island of Beguia, and it was alledged there to have been brought from Guadaloupe. One of my patients brought it from Beguia, and died on the 11th day. Soon after, a brother of Dr. French, of St. Vincent's, was attacked with fever. The remissions were at first very evident, observing the double tertian type. The usual evacuations being premised, the bark was thrown in, but without any good effect, as it disagreed with the patient's stomach. All his symptoms increased, with delirium, subsultus tendinum, quick low pulse, *dry skin, and great heat.* The cold bath was administered. After drying the surface and putting the patient to bed, a dose of tincture of opium was given in warm wine and water. He got into a profound sleep, with profuse sweating, and next day was in a condition to take the bark. A repetition of the bathing completed the cure.

" I had three other cases at the same period; two of which were attended by my lamented and much esteemed friend, Dr. Mackie, of St. Vincent's, who despaired of their recovery. But the cold bathing proved successful in them all."

In these cases Dr. Davidson used cold bathing in the advanced stages of the fever. He however mentions, that the heat was still great, and the skin hot, and to these circumstances his success is no doubt to be attributed. In the malignant yellow fever, which raged in St. Vincent's, and the neighbouring islands, in 1793, Dr. Davidson changed his plan of administering the remedy, of which a more distinct account is contained in a letter, which he wrote at that time, to Dr. Brown, of Baltimore, than in his letter to Dr. Robertson; all of which, as it is given in an American publication of 1794, we shall therefore avail ourselves.

It appears, that Dr. Davidson, having heard of the great mortality in Philadelphia, in 1793, from the malignant yellow fever, thought it might be useful to communicate his experience of the proper mode of treatment of a similar disease, which had recently occurred in St. Vincent's. "I have observed with uneasiness and concern," says Dr. Davidson to Dr. Brown, "that an epidemical fever, nearly similar to that which we have had in this island, for six months past, has appeared in Philadelphia. The disease, as it occurred with us, was marked with more violence, and proved more fatal, than I ever recollect to have seen in any other

other instance, during a residence of twenty-two years in the West Indies, and part of that time at St. Lucia, where I had an opportunity of seeing the fever among the troops, and sailors, in all its forms.

“ Early in April, the yellow fever made its appearance, both on shore, and on board the shipping. The young, plethoric, and those lately arrived from a cold climate, were chiefly affected. The inflammatory symptoms ran high. Blood-letting, blisters, and gentle and constant purges, answered at first; but in the farther progress of the disease, the weather still continuing hot, the marks of inflammatory diathesis became less evident and the pulse sunk on blood-letting. The vomiting did not appear on the first days, and sooner ended in black vomiting. We found it necessary to alter our practice; but I must candidly confess, that till I adopted the use of cold bathing, under the circumstances which indicated a typhus type, our endeavours to cure the fever were attended with little success. In the inflammatory state the disease was more at command, but in the other, the irritability of the stomach was such that we could not use tonics. Neither bark, wine, nor opium, could be retained. Upon a general review of the ill success which attended the

practice of the medical gentlemen in this
 and, as well as in Grenada, and Tobago, I was
 inclined to try the effects of cold bathing, which
 had used with success in the advanced stages of
 intermittent fever. At first I tried the effects of cold
 bathing in the advanced stages of this fever also,
 without success; but as every other plan
 finally failed, I began with the *cold bathing in*
commencement. Warm tamarind tea, or cream
 of tartar and manna beverage, was given imme-
 diately after the application of the cold bath, to
 induce sweating, and to open the body, if this last
 effect was not already produced by clyster; and
 as soon as a sweat appeared, bark, mixed
 in the beverage, was given in as large quanti-
 ty as the stomach would retain, without paying
 attention to the fever, or state of the patient's
 pulse. When, however, the stomach rejected the
 bark, and there appeared to be an increase of head-
 ache, heat, and other symptoms of febrile affection,
 I had again recourse to the cold bathing alone,
 which was commonly repeated evening and morn-
 ing, till the patient was out of danger. *I am*
happy to announce, that this mode of treatment
has been attended with the utmost success. The
 cold bathing seemed to take off the determination
 from the brain, to remove irritability, and to deter-
 mine

mine to the surface, for in every case, a plentiful sweat was excited*.”

Dr. Davidson's narrative has not all the precision that could be wished, yet it is sufficiently clear in the points most essential, and particularly in this, that in a fever of the most fatal nature, where all the established modes of practice failed, and where the cold bath itself equally failed when used in the latter stages, it was attended with the *utmost success when resorted to in the commencement of the disease, and duly repeated as the fever returned.*

Unfortunately cold bathing had been already proposed, as a remedy for the fever of Philadel-

* This letter of Dr. Davidson, to Dr. Brown, was by him sent to Dr. Cumming of Philadelphia, by whom it was communicated to Dr. William Currie, of the same city, who has published it in a note to his “Impartial Review,” of Dr. Rush's “account of the bilious remitting yellow fever, in Philadelphia, in the year 1793.” The letter of Dr. Davidson, appears to be given chiefly with the view of showing the existence of a similar disease in the West Indies to what appeared in Philadelphia, and the probability therefore of this last being imported. Dr. Currie makes no remarks on the practice of Dr. Davidson, nor does it appear to have been imitated either by him, Dr. Cumming, or Dr. Brown.

ia, in the beginning of the epidemic of 1793,
Dr. Edward Stevens, of St. Croix*.

Dr. Stevens recommended the use of the cold
bath every morning, in the state of languor and
prostration, which ushers in the disease, with a free
dose of Madeira wine, a gentle opiate at night,
solid diet, and rest.

“ When the disorder has gained ground,” says
Dr. Stevens, “ and become violent, and when the
danger is imminent, the most unremitting exertions
should be made by the physician, to mitigate the
symptoms. The nausea, and vomiting may be
relieved by an infusion of chamomile flowers,
taken frequently, until the stomach is sufficiently
cleared of all crude matter ; small doses of a
cardiac mixture, composed of the oil of peppermint,
and compound spirits of lavender, may then be
taken until the fever abates. If, notwithstanding,
the irritability of the stomach should still continue,
the course must be immediately had to the cold bath,

* The same who published a very ingenious inaugural
dissertation at Edinburgh, in 1777, “ De alimentorum con-
tractione,” in which are contained some curious original ex-
periments made on the person who swallowed stones for his
relief.

which

which must be used every two hours, or oftener, if the urgency of the symptoms should require it. After each immersion, a glass of cold Madeira, or a little brandy, burnt with cinnamon, may be administered." Dr. Stevens goes on to order injections of bark and laudanum, and fomentations of flannels wrung out of spirits of wine. As soon as the state of the stomach will admit, he proposes bark, wine, and nourishing food, dissuading from purging, blood-letting, and every part of what is called the antiphlogistic treatment. "Upon the whole," says Dr. Stephens, "I may sum up this hasty outline, by inculcating the use of the tonic plan in its fullest extent, and by warning against the ill consequences of debilitating applications, or profuse evacuations in any period of the disease; the cold bath, bark, and wine, a spacious well ventilated room, frequent change of bed and body linen, and attention to rest and quiet, will, in most cases, prove successful, and strip this formidable disease of its malignity, its terror, and its danger."* It appears that the plan of treatment proposed by Dr. Stevens, was in some instances successful. It was

* See Dr. Stevens's letter to the president of the college of physicians, of Philadelphia, dated September 16, 1793, inserted in Dr. Rush's "Account of the bilious remitting fever, in Philadelphia, in 1793," p. 223.

strongly recommended to the public, in a printed letter, from the celebrated Mr. Hamilton, then secretary to the treasury, he having experienced its benefits in his own person.* The plan of Dr. Stevens, was at first adopted by Dr. Rush. "I began," says he, "the use of Dr. Stevens's remedies the next day after my interview with him, with great confidence of success. I prescribed bark in large quantities; in one case I ordered it to be injected into the bowels every four hours. I directed buckets full of cold water to be thrown upon my patients. The bark was offensive to the stomach, or rejected by it in every case in which I prescribed it. The cold bath was grateful, and produced relief in several instances, by inducing a moisture on the skin. For a while I had hopes of benefit to my patients, but in a few days, I was distressed to find that these remedies were not more effectual than those I had previously used. Three out of four of my patients died, to whom the cold bath was administered in addition to the tonic remedies before mentioned."† In consequence of this bad success, Dr. Rush was induced to abandon the plan of treatment, of Dr. Stevens, and to adopt an opposite method;—the free use of evacuants, particu-

* See Dr. Rush's "Account," &c. p. 214.

† See Dr. Rush's "Account," &c. p. 195-6.

larly bleeding, and purging, to which he has given the name of the depleting system. On the propriety of this system, I do not enter, being most unwilling to touch on the controversies in which it has been involved; but I may be permitted to say, that the cold bath, used according to the directions of Dr. Stevens, could not, on the principles I have laid down, be attended with success, except, under the accidental occurrence of circumstances which were not in his contemplation, and which his precepts would lead those who employed it, rather to avoid than to embrace. Instead of recommending the cold bath in the first attack of morbid heat, Dr. Stevens does not seem to advise it as a remedy, till the disease has "gained ground," and the "danger is imminent." If nausea, and vomiting, can be relieved by no other means, recourse must be had to the cold bath, which is to be used *every two hours, or oftener, if the necessity of the symptoms* (that is, of the nausea, and vomiting) *should require it.* One can suppose, that in certain vigorous constitutions, even such an application of this remedy might be successful, but generally speaking, it must be expected to be hazardous in the extreme. Its comparative success, and safety, in the hand of Dr. Stevens, in the fevers of St. Croix, we must ascribe to their being of a milder nature, and perhaps in
part,

part to the water of the springs on the shores of that island, being twenty degrees warmer than in the wells of Philadelphia. In a subsequent publication of Dr. Rush, we learn, that the use of the cold bath was afterwards abandoned by Dr. Stevens himself. "Dr. Stevens," says Dr. Rush, "recommended bark, wine, and the cold bath, for the cure of the yellow fever of 1793, because he had seen these remedies most effectual in the yellow fever of St. Croix, but he laid all these remedies aside, and chiefly relied on a salivation, in the cure of the fever, of 1797, of Philadelphia."*

Dr. Rush, however, in the bilious yellow fever of 1794, used partial applications of cold water to the surface of the body, and also employed it in the way of clyster, with good effects; † but he did not resort to the general affusion, or push the remedy to the extent necessary to reap its full advantages. On other occasions it has been used in the destructive fever which has prevailed in the great cities of America, particularly by Dr. Baylis

* Second address to the citizens of Philadelphia p. 29, by B. Rush, M. D. 1799.

† An account of the bilious yellow fever in 1794, p. 91. by B. Rush, M. D.

of New York ; but so far as I know, similar observations are applicable to the mode of using it in such instances, as to that laid down by Dr. Stevens. It has in general been used as a stimulant and a tonic, in the advanced stage of the disease ; no regard has been paid to the patient's heat ; nor do I know that a single thermometrical observation has been made on this dreadful fever, by any of the physicians of America. In my view of the subject, we are still uninformed in regard to the most important particular connected with the history of the disease.

The first edition of the Medical Reports had no influence that I know of, on the mode of practice in Philadelphia, or New York, in the fatal epidemic of 1798. The practice I had recommended met indeed the approbation of Dr. Eustis, of Boston ;* and the second edition of the work was abridged, and recommended to public notice, by a gentleman, in the district of the Maine, formerly a member of the British House of Commons.† But in the middle states, where the mortality has chiefly prevailed, other modes of practice have en-

* See his letter, dated November 21, 1798, p. 18, of the first number of the Medical and Physical Journal.

† Mr. B. Vaughan.

grossed the general attention. The Medical Reports was announced, but neither reviewed nor analyzed, in the Medical Repository of New York. From a recent number of this valuable publication, I however see with pleasure, that the practice I have recommended, begins to receive some share of attention. The following is the extract of a letter from Dr. Selden, and Dr. Whitehead, of Norfolk, in Virginia, to Dr. Miller, of New York, dated July 15th, 1802, and published in the 6th volume of that Journal. After a general view of the causes, and of the symptoms of the yellow fever at Norfolk, in 1801, they proceed as follows.

“The plan of treatment proposed this year was, in many respects, similar to that adopted in 1800, of which we have formerly given some account. The lancet, however, was more sparingly employed; calomel, in all cases, was liberally exhibited, both with a view to produce, in the commencement, a full and speedy evacuation, and afterwards also, in such forms as have been found to bring on most readily a salivation, which, in every instance, with us, as has been often noticed by others, was followed by the recovery of our patient. Where topical affections occurred, we had recourse to local remedies. Cupping, and venesection, were early employed, afforded frequently great relief. Neither theory nor experi-

ence warranted the early exhibition of the bark ; we always deferred it till some change in the febrile symptoms began to appear, and the irritability of the stomach had abated. But under every form of treatment, numbers fell victims to the disease. In this juncture, being desirous of making every effort, we had recourse to a remedy we had last year tried in a few cases with some benefit, and now found attended, as far as it was carried, with unequivocal success. This was the liberal affusion of cold water, not on the plan prescribed by some of the writers of the West Indies, but in a mode similar to that recommended by Dr. Currie of Liverpool.

“ The first trials were made on young robust British seamen, and the good effects of this remedy equalled our most sanguine expectation. After the affusion of the cold water, the pulse was often thereby reduced thirty strokes in a minute, the burning heat of the skin was greatly lessened, and the thirst, headach, and other uneasy sensations, were greatly alleviated. The patient generally found himself so much relieved and refreshed, after *that* the cold bath, ~~he~~^{he} submitted, not only without reluctance, but with pleasure, to a repetition of it.

“ If called in on the first or second day of the attack, we first directed a strong dose of calomel
and

and jalap, in order to procure a full evacuation of the bowels, after which the patient was ordered to be carried on deck, with only a great coat thrown loosely around him, and three or four buckets of salt water to be poured on his head and naked body. The operation was repeated, when the febrile symptoms threatened to return with their former violence. Three times a day was usually sufficient. We rarely found it necessary to continue the use of the cold affusion longer than the fourth day; during which time the bowels were kept open by the occasional exhibition of a bolus of calomel.

“From the great benefit experienced in the two or three first trials, we proceeded to recommend it with confidence. *Of all those patients to whom we had an opportunity of exhibiting this remedy, on, or before the second day of the attack, we had the good fortune not to lose one;* but after this period, when the fever had begun to subside, without symptoms of amendment, the affusion of cold water seemed only to hasten the fatal catastrophe. In no instance was it used without the exhibition of calomel at the same time, and we might have been inclined to ascribe to calomel, the merit of the cure that was accomplished, had it not failed with us, sometimes, under the fairest trials.

“ No disagreeable effect was produced by combining the use of calomel with the affusion of cold water; nor in a single instance did the mercury procure a salivation, although the discharge from the bowels was scarcely as great, as when it was used alone in the cure of the disease. But, in almost every instance which terminated favourably when the cure was trusted to calomel without the cold bath, some degree of salivation came on, and in such cases the appearance of this discharge was beheld with pleasure, being regarded as an infallible mark of safety.

“ It is with the fullest conviction of the superiority of this plan of treatment, to any we have yet tried, that we record its effects. The subjects of our experiments were those in whom we found the disease to attack with the greatest violence in the first commencement, and to act with the most fatal force on their constitutions. We shall attempt no theory of the manner in which the salutary effects of cold bathing in yellow fever are produced, nor venture to recommend it as a certain remedy; but we think, that in the hands of a skilful and judicious physician, it may often prove a powerful auxiliary, in enabling him to combat the fatal effect of that dreadful calamity we have been describing.”

After

After the body of evidence which has been brought forward, and particularly after this narrative, I may perhaps, though a stranger be accused for proposing a more general trial of the practice recommended in this work, to the candid and unprejudiced physicians of America. Proposals for improving the method of cure of the destructive fever of that country, cannot be considered as superfluous. "Four times as many persons," says Dr. Rush, "were affected by the yellow fever of 1793, as in 1798, but the mortality of the two years was nearly equal;" a melancholy truth, which affords room for much serious reflection. Nor has the subsequent experience of American physicians, discovered a mode of treatment, on which confidence rests. The great cities of America, are still deserted on the appearance of the yellow fever, which excites every where alarm and dismay. The method of treatment which we have proposed, should not be rejected, as being which has been already tried there, and found efficacious. An attentive consideration of cir-

Dr. Rush's second address to the citizens of Philadelphia, 1799, published in 1799. Dr. Rush imputes this vast increase of the proportionate mortality, to the desertion of blood-letting. Perhaps it may have been in part, owing to the cessation of cold bathing, by which, though used at random, many seem to have been saved in 1793.

cumstances

cumstances will shew, that the former trials of the external application of cold, could not, on the principles here laid down, be attended with general success; and that though at times such trials may have succeeded, they must on the whole have been precarious and dangerous. The evidence here produced, shows the singular success of this application, regulated by these principles, in febrile diseases, in various regions of the earth, and more especially, in countries and climates, resembling those of the United States of America. That it will succeed there also, is a fair presumption, which nothing but the result of careful and continued experience should be allowed to overthrow; since the evil is one, for which no other remedy has been found, and, since it is of a magnitude to obstruct the high destinies of a people, otherwise most happy and prosperous.

I venture these observations with hesitation, and offer them with deference and regard. An observer at this distance must be particularly liable to error; and those who have performed their duty with courage and fortitude, amidst scenes of unprecedented toil and danger, are entitled to the respect, as well as the sympathy of their more fortunate contemporaries.

CONCLUSION.

CONCLUSION.

It would not become me to conclude without some notice of Dr. Wright, with whose important narrative this publication commences.

This respectable physician, after having retired from the fatigues of his profession, had his services called for, once more, by Sir Ralph Abercrombie, and attended the last West Indian expedition of that illustrious and lamented commander, in the quality of physician to the army. On his return to Britain, he landed at Liverpool, in June 1798, and I had then an opportunity of forming, not merely an acquaintance, but a friendship, with one whom, while unknown, I had been so much indebted. I found in Dr. Wright, an excellent physician and naturalist, who had devoted a long life to the pursuits of science, not in academic bowers, but in situations of toil, difficulty, and danger; who had profited of his ample experience by constant and unprejudiced observation; who possessed a generous and disinterested temper, and a simplicity of manners, worthy of a

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more virtuous age. From that time he has resided in Edinburgh, and I have had the advantage of his regular correspondence, and of his valuable observations. He has been uniformly zealous in promoting my medical pursuits, and to his kindness, I owe the acquaintance of Dr. Macneill, Dr. Robertson, and Mr. M'Gregor, by whose communications I am so much obliged.

During his last residence in the West Indies, and while director of the military hospitals in Barbadoes, Dr. Wright drew up for the Medical Board in London, a report on the diseases most common among the troops in the West Indies. In speaking of the cure of the ship fever, he observes, "in the beginning of the ship-fever; the cold bath had the best effects; and through the day, when the sick were hot, washing the hands and face suddenly in cold water and vinegar, was exceedingly refreshing." In like manner, in treating of the yellow fever, he remarks, "in the beginning of the yellow fever, the cold bath succeeded admirably, but in the advanced stage, much caution was necessary." I quote these sentences from a report, the whole of which deserves the most careful attention of military practitioners in warm climates, to show that the experience of Dr. Wright, continued to justify his original recommendation

commendation of the cold bath in fever, and to testify in particular, the mode in which I had recommended it, at a time when my publication was equally unknown to him, as his report was to me.

In a few months after his visit to Liverpool, I received from Dr. Wright, his remarks on the second edition of the Medical Reports, much at large. In these, after supporting all the principal parts of my treatment of fever and convulsive diseases, from original observations of his own, he concludes by assuring me, that my work has his unqualified approbation. In subsequent communications from this venerable physician, he informs me of the success attending his use of the cold affusion in febrile diseases, in Edinburgh; particularly in the late influenza, which he treated as a fever of debility, allowing a liberal diet, and the moderate use of wine, but keeping down heat and flushings, by the sudden application of cold water on the surface; a mode of treatment which he found invariably successful; and he expresses a confident opinion, that the cold affusion, well timed, will not only cure all febrile exacerbations, but prevent their taking place. "I agree," says Dr. Wright, "with Dr. Falconer of Bath, in thinking, that the cold affusion will secure persons

sons from taking the plague itself, though exposed to its contagion.”

In his different communications, this liberal physician gives me every credit for having ascertained more precisely the rules by which the application of cold to the surface should be regulated, and particularly for the introduction of thermometrical observations into the history of diseases, a practice before, either unknown or neglected. My observations on the change of animal heat, and particularly on the changes produced on it by cutaneous perspiration, (the origin of which is given, vol. i. *p.* 247,) had prepared me for tracing the changes it undergoes in fevers, previous to the appearance of Dr. Wright's narrative, and led me insensibly to detect that generally prevailing error of modern times, that the application of cold to the body, externally or internally, is dangerous in proportion as the body is heated; more dangerous therefore in fever than in health; and most dangerous in that state of fever where the heat is greatest. The detection of this error, necessarily connected itself with a true theory of the principal function of the perspiration, that of regulating the temperature of the body in health and disease; a most important function, previously disregarded in the writings of our physicians on fever, and wholly overlooked in the works of what are called the systematic authors in medicine,

ne, not excepting the latest of them, Cullen, Brown, and Darwin*.

That some advantages are to be obtained from strict attention to the state of the heat in fever, and to the proper function of the perspiration, this volume, now submitted a third time to the public, affords, if I do not deceive myself, important proofs. A careful attention to the changes of the animal heat, and to the state of those

* This function of the perspiration, or transpiration, had been equally overlooked by the physicians of the continent. The chemical physiologists did not wholly pass it over. In vol. i. p. 272, the speculation of Franklin on this subject is mentioned; and in the year 1790, the celebrated Lavoisier read before the academy of sciences, a memoir on transpiration, the joint production of Séguin and himself, the principal object of which is to shew, that this discharge cools the system, and regulates the living heat, which respiration had produced. This memoir, with the other papers which constitute the volume of the academy for 1790, the last of the series, was not published, in consequence of the revolution, till 1797, and was wholly unknown to me till 1801, when the short-lived peace enabled us to obtain the products of the French press. I was not a little gratified in finding that my observations on this subject, on which I had built so much, were supported by such high authority. So far as I have been able to examine, no abstract of the memoir on transpiration was published at the time of its being read, or before 1797, in any of the French journals.

functions

functions on which it depends, and by which it is regulated, though more requisite in febrile diseases perhaps than in others, is however of importance throughout the whole circle of disease. The capacity of the animal to preserve its heat nearly unchanged in the various changes of temperature of the surrounding media, is essential to, may indeed be said to constitute life. Digestion, respiration, and perspiration, these are the three functions, on which the living heat, and life itself immediately depend, and by the relative actions of which the just medium of the animal temperature is preserved. To mark the various changes in these strictly connected functions, and compare them with the actual changes in the animal temperature in the different circumstances of the living system, might appear a humble labour, because requiring little but patience and attention. Yet on such a labour we can conceive that Bacon might have entered with alacrity; in such a labour Lavoisier was actually engaged: conducted patiently and carried on perseveringly, such a labour might terminate in the most important consequences; might give us just principles of the effects of temperature on life, and after the fruitless wanderings of the human mind for three thousand years, might lead to a true theory of health and disease.

In the progress of such an investigation, the effects of different kinds of food on the actual state of the living heat, and on the faculty of preserving the healthy temperature, would come under examination; the opinions suggested by a false theory on this subject would be detected, and the regimen of diet be improved.

The effect of different medicines on the animal temperature would likewise be submitted to experiment; a new light would be cast on their mode of operation; and more accurate rules laid down for their administration.

The connection between the state of respiration and the actual temperature, certainly not an obvious one, would probably be discovered; and that between temperature and perspiration be distinctly and easily traced.

And, to pass over a multitude of other suggestions that crowd upon the mind, the question respecting the existence of that morbid stricture in the extreme vessels of the surface, by which perspiration is at times impaired or obstructed, heat accumulated, and morbid action induced, could be brought to issue, and one of the most important of the phœnomena of disease, be in my judgment, clearly ascertained.

But to succeed in such an investigation, it seems necessary that the phenomena of these different functions should be examined, with a reference to each other; by which each series would be illustrated, and the general results rendered clear and certain.

Leisure for such an investigation, even if the other qualities were not wanting, cannot be hoped for by one whose time is so much occupied, and whose health is so precarious. But under each of these heads, I have collected a few observations, which I shall one day submit to the public, should my life be prolonged. In the mean time, returning to the more immediate object of this publication, I shall conclude with the following remarks.

Though I am far from thinking that fever, properly so called, consists merely of a series of phenomena originating in a morbid accumulation of heat in the system, yet this symptom evidently occurs, more or less, early in that disease; and it will not now, I think, be denied, that the sudden abstraction of this heat, if done early, and without debilitating the patient, very often, if not generally, puts an immediate end to the pain; and that even in cases where the termination is not immediate, the symptoms are by this means abated, and the fever brought to a more speedy issue.

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The cold affusion so far from being a debilitating, may indeed be considered as an invigorating power, which, by the sudden, general, and powerful stimulus which it communicates to the system, destroys the morbid stricture on the skin, and restores that process to its due activity, by which the future accumulation of morbid heat is prevented. It seems at the same time to interrupt effectually the actions of fever, and to restore those of health.

Though the morbid heat be not considered as the original cause of fever, yet it will be evident, on a slight consideration, that no remedy can stop fever, which suffers the morbid heat to continue. I have found by experiment, what indeed might have been safely inferred without it, that a heat, four or five degrees greater than that of health, however induced, cannot exist in the living body, without being attended by increased rapidity of circulation, increasing debility, and all the principal symptoms of fever. If this be true, all other remedies which have been supposed to stop fever in its early stages, whatever their other qualities be, must have directly, or indirectly, the power of diminishing morbid heat. These remedies are, emetics, sudorifics, and blood-letting, of which I find, by experiment, are possessed of this power, though in different degrees.

Without referring to the origin of the febrile heat, it may be laid down as a rule, that in febrile diseases it is so far connected with increased vascular action, that whatever reduces this action, reduces the heat. The nauseating, and debilitating effects of antimonial emetics, produce this diminished vascular action in a striking manner, and reduce the heat of the surface, as I have found by the thermometer, in a corresponding degree; and not of the surface only, but of the whole system, if the sensations may be trusted. They also increase the cutaneous perspiration, and stop for a time, the febrile actions. Sometimes, especially when used very early, (for it is only in the first stages of fever that antimonials are useful, or even safe) they interrupt the disease effectually. But much more generally, after the sickness passes off, the fever returns, and returns to the frame very considerably debilitated. If the remedy be repeated, the chance of benefit is diminished, and the debility greatly augmented. Antimonials are more debilitating than ipecacuanha; and remedies of this class are more salutary in inflammatory, than in contagious fever. Sudorifics act less powerfully than emetics. But when used early, and duly persisted in, they are sometimes effectual in stopping fever; though more generally useful in alleviating its symptoms.

They are useful however only while the morbid heat continues, or the disposition to it exists. That they act by diminishing this heat, is evident; but they also diminish the strength.

Blood-letting is a still more refrigerating remedy, and seems only useful in fever when it produces this effect. It operates no doubt by the sudden diminution of vascular action. I have so seldom used it in contagious fever, that I cannot speak of direct experiments to establish its cooling effect in that disease. But in inflammatory fever, and more especially in that connected with inflammation of the lungs, I have often observed the rapid diminution of the heat as the blood flows, with admiration. Such experiments I have frequently had an opportunity of making in my own person. In one instance, where my heat was 103° of Fahrenheit at the moment the lancet was introduced, and I watched, as usual, the mercury of the thermometer, the bulb of which was grasped in the burning palm of my hand, after a few seconds I perceived it began to fall—at first slowly—and afterwards rapidly. I saw it sink to the 91st degree, I was then cold all over me, my forehead, and surface, were bathed in a profuse sweat—at this moment delirium supervened upon me, but the bulb of the thermometer

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being still in my hand, my assistant found the mercury to sink to 83° before he could bind up my arm. On weighing the blood, it appeared that I had lost about eighteen ounces.

I can conceive, that in some fortunate instances, the early use of this remedy may have stopped the progress of fever, or brought it to a speedier issue. But that it has not usually such an effect is certain, and the great debility which it occasions, must impair, or destroy the chance of recovery, where the fever proceeds in its course. In some cases, the moderate use of this remedy may have co-operated with the cold affusion, but where the febrile heat can be removed by any mode of the application of cold to the surface, it is far safer to trust to this alone.

Of this last remedy it can alone be said, that while it subdues the principle of fever, it does not impair, but invigorate the powers of life; that it may be repeated again and again with safety; that its application admits of precise rules; and that while it is the most powerful of the means of cure, it is the best preventive against the spreading of the disease. In the warmer climates where the stomach and bowels are so liable to be affected in fever, calomel appears from experience

to be the most useful remedy, in combination with the application of cold to the surface. It operates, no doubt, by clearing the stomach and intestines, and thus removing morbid irritation, and abating morbid heat. It has been thought—though this is less certain—that the specific effects of the mercury on the system, are equally, or more important, and respectable physicians have asserted, that where salivation could be produced in the yellow fever, recovery was almost always the consequence. It may be said, perhaps, that where the power of the absorbents remains, and there is time for salivation to be effected, the disease has been originally of a milder nature, and the recovery more probable from the first. But I may also remark, what has not before been observed, that salivation is accompanied by a more profuse perspiration from the surface, a circumstance which may diminish the febrile heat and irritation.

By the help of observations such as these, we might, perhaps, be enabled to explain the contradictory reports of the effects of remedies by respectable practitioners, in the rapid and destructive fever to which the West Indies and America have of late been so particularly subject. But it is better to leave their application to those candid

and judicious observers, who alone are accessible to the opinions of others, and capable of bringing them fairly to the standard of truth.

Liverpool, 6th May, 1804.

CHAP. VI.

Additional information respecting the use of the cold and tepid affusion of water in fever, since May, 1804.

ABOUT the time that the last sheet of the third edition of the Medical Reports was printed, (May, 1804) I received a letter from Dr. Barry of Cork, accompanying a copy of the annual report of the hospital for fever in that city, entitled the House of Recovery, in which a new and striking evidence in favour of such establishments is given to the world. In the letter alluded to, Dr. Barry mentioned to me generally, the great success which had attended the affusion of cold water, during the preceding year, in the practice of that hospital; and afterwards at my request furnished me with details of twenty cases of fever, and three of influenza, in which he had trusted the cure chiefly to this remedy. The same post brought me a similar communication from Dr. Daly, the colleague of Dr. Barry, containing the particulars of nineteen other

other cases of fever in the same hospital under his care, in which he had adopted the same practice with equal success.

At an early period of the history of this remedy, I should have thought it my duty to have given these very satisfactory cases in detail. After the accumulation of evidence on the subject of this practice, that has been laid before the public, it will be sufficient to speak of them generally.

The application of the cold affusion was regulated very exactly by the principles laid down in this publication. The effects were on the whole such as former experience would lead us to expect. All the cases terminated favourably. Dr. Barry, laments that they had so few opportunities of using the cold affusion, within the period in which it usually cuts short the disease. In six of the cases, however, detailed by him, and in four of those of Dr. Daly, "the progress of the fever was effectually interrupted, and the patients were quickly restored to health." In some of the cases thus cut short, the fever had existed four, five, or even six days; but the experience of Dr. Barry, and Dr. Daly, confirms the position, that the earlier the cold affusion is applied after the morbid heat is established, the better the chance of the immediate termination of the disease. Even in the cases in which the fever

an its course, from the remedy not being resorted to in time, "the symptoms," says Dr. Barry, "were considerably mitigated, the patients feelings were rendered much more tolerable, and the advances to health became more steady and certain."

The effects of the cold affusion, in diminishing the heat, in lessening the frequency of the pulse, and in producing a disposition to sleep, were such as have already been so often described. In two or three instances, however, on applying the thermometer after the cold affusion, Dr. Barry found the heat not diminished, but rather increased, "though the pulse had fallen, and the more distressing symptoms were relieved." This relief did not in such cases prove permanent; the fever continued its course.

The dread which some of the sick had at first of the cold affusion, seemed an obstacle to its good effects. Under this dread the pulse was not always reduced by it in frequency, and the patients sometimes trembled for a short time after going into bed. But when they became accustomed to the shock, which generally happened on the second or third application of the remedy, it had its usual good effects.

The *lavatio frigida*, (wetting or sponging the
surface

surface of the body) was had recourse to, in a still greater number of cases than the cold affusion, and with advantage. Its effects, however, were much inferior to those of the cold affusion. The heat was seldom so much reduced by it as to make any change in the pulse, though the patient felt much relieved by it. It was chiefly used in those cases which from weakness seemed unfit for the cold affusion. The auxiliary remedies used in these cases, were chiefly calomel to keep the bowels regular, anodynes when required, and in some instances a moderate quantity of wine.

The cases of influenza, (which are particularly detailed) occurred in the spring of 1803. The symptoms were of the most violent kind. The cold affusion proved strikingly salutary, in spite of considerable pulmonic affection.

The cold affusion has been used in several other parts of Ireland with success, but the particulars have not come to my knowledge. There is no doubt that it will prove equally salutary there, as in the sister island.

It was my fate to spend a considerable part of last winter, (1804-5) at Clifton near Bristol. Accounts were received there in the month of January, of the appearance of a contagious fever of

malignant kind, among the French prisoners at Stapleton in that neighbourhood, and as is usual in such cases, its fatality was much exaggerated. Desirous of offering any assistance in my power to these unfortunate men, I made a visit to Stapleton, and called on Dr. Jeffcott, the gentleman who has the medical superintendence and care of the prisoners under the Board of Sick and Hurt. Dr. Jeffcott, was at that moment absent on business, but on mentioning my name and the object of my visit, I was received with great civility by his assistant, by whom I was conducted through the hospital for the sick. It contained at that time upwards of 200 cases of typhus, in different stages of their progress; each of the patients so far as I observed had a separate bed, and was comfortably provided with bed-clothes and linen. The arrangements respecting food and medicine seemed liberal, and the attendance of every kind exceeded what might have been expected, on so difficult and sudden an emergency.

The fever was evidently typhus, in a malignant form. In such patients as I examined there were, head-ach, dull, and sometimes blood-shot eyes, much disturbance of the functions of the mind, great prostration of strength, and very generally petechiæ. The pulse was not very strong, in frequency it ran from 90 to 120 in the minute; the skin

skin was dry ; the heat various in different patients. I had not an opportunity of measuring it, but it did not seem immoderate in any, the state of fever considered. From the gentleman who accompanied me round the hospital I had every information I required, and Dr. Jeffcott himself who returned my visit at Clifton, has been so obliging as to give me a written answer to my queries since the epidemic was subdued. I am thus enabled to state the following particulars.

The contagious fever was first observed among the prisoners at Stapleton on the 10th of January (1805) ; the last case received into the hospital, was on the 20th of April. The whole number of cases of fever, was eight hundred and fifteen, of these between three and four hundred were cut short by the cold affusion, many of them in the second, third, and fourth days of the disease. From four to five hundred ran their course, and of these forty-one died.

The whole infectious period lasted a hundred days, and the number received on an average into the hospital, was rather more than eight daily. But the actual number received in a given space of time varied much. It was greatest in the second and third weeks of February. On the 15th of that month, eighty-five patients were received into the hospital,

hospital, none of whom had any symptom of disease upon them the preceding day at one o'clock, the hour at which all the prisoners were daily examined by Dr. Jeffcott, and when all that complained were sent to the hospital. The fever attacked the old and young, but the young more particularly. These, however, almost all recovered. The victims were chiefly men advanced in life, and of worn out constitutions. The system of treatment followed by Dr. Jeffcott, I will give in his own words, extracted from his letter to me of the 7th May, 1805.

“Immediately on the patients being sent to the hospital, and before they were put into their respective births, they were put into the warm bath, and soap was used to cleanse the skin effectually. They were then wiped dry, clean linen was put upon them, and they were put into bed. There they were left in general two hours, when their pulse and heat were examined accurately by the watch and thermometer as directed by you, and where the pulse exceeded one hundred, the skin being proportionably hot, without evident marks of pulmonic inflammation, the patients were put into an empty tub, and two or three buckets of cold water, each containing from three to four gallons, were dashed over their heads and bodies with the happiest effects. This was repeated two or three times or more as
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the symptoms indicated. About four hundred were effectually cured in this manner, some of whom were free of fever on the 2d, 3d, and 4th day. In the advanced stages instead of the affusion, the body was sponged all over three or four times a day.

“Salines, calomel and wine were given during the fever, as required, and bark in the convalescent stages.

“I had frequently in the course of my practice on board ships of war, experienced the good effects of the cold affusion, as recommended by you, and was therefore prepared for its use in this epidemic. The French prisoners were at first offended at being sluiced with cold water, but they soon called aloud for it. Their continual cry was, “*Les Anglois coupent la fièvre d’instant par le moyen des bains froids.*”

One symptom which occurred toward the latter end of the disease, where it ran its course, was a livid colour of the feet and toes, which afterwards became sphacelated in spite of every stimulant that could be applied to the part or given internally.

“The cold affusion or immersion was not used as a preventive to prisoners in health. Five of the attendants on the hospital caught the fever, two of whom died.

“ I fumigated

"I fumigated the wards of the hospital continually with the muriatic acid gas with good effects. The prison has been fumigated repeatedly with the nitrous acid gas, after the plan of Dr. Carmichael Smyth; and constant fires of wood were kept in every ward, the strictest attention being paid in all other respects to cleanliness and ventilation.

"The different days of the fever on which the patients died, so far as could be ascertained, are as follows :

| | | | |
|---------|--------|--------------|--------|
| 1st day | 1 died | bt. forward | 19 |
| 3d | 2 | 12th day | 5 died |
| 7th | 4 | 13th, 14th | 2 |
| 8th | 3 | 16th | 2 |
| 9th | 5 | 17th | 1 |
| 10th | 3 | 18th | 4 |
| 11th | 1 | 19th to 30th | 8 |

car^d. forward 19

in all 41

"On referring to Dr. Cullen, the deaths here do not correspond with his critical days."

"On this narrative I will offer a few reflections. Though the success of Dr. Jeffcott in this epidemic is not equal to what occurred in some other epidemics where the cold affusion was employed,

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yet if all the circumstances of the case be considered, it will be found very considerable, and probably unparalleled in the case of any epidemic fever among the same description of patients.

Where fever originates among prisoners of war, it must necessarily attack many persons of broken and worn-out constitutions. This was certainly the case at Stapleton; as I passed through the wards of the hospital, I noted the worst cases that presented themselves. These were men rather advanced in life, and the few whose history I enquired into had been debilitated by service in the West Indies.

When a contagious fever originates among prisoners of war, the subjects of it are unfavourably situated in various other respects. They have necessarily been for a considerable time before exposed to the pre-disposing causes, all of a debilitating nature. These causes may be easily enumerated; a slender diet—depression of spirits—bad and filthy clothing—foul air, from being too much crowded together—and a great want of personal cleanliness. It is to be observed that prisoners of war are sometimes too much crowded together, not from want of sufficient space within the walls of the prison, but from want of a sufficient supply of fuel and clothing to protect them against the inclemency of the weather. Hence they voluntarily collect to-

gether

together in a very inadequate space, that they may profit by the warmth of the air of respiration, without being aware that the same process that warms it contaminates it also. The house for the reception of prisoners of war in Liverpool was a noble prison, built by the Corporation of that town, for the purposes of solitary confinement after the plan of Mr. Howard. When I visited that prison in the winter of 1800-1801, I observed that in a cell destined for a single prisoner, there were often eight or nine Frenchmen crowded together, which was twice as many as was requisite, supposing the whole to have been distributed equally. On enquiring into the cause of this, I was told that they were not allowed fuel, except in the cooking kitchens; that they were without sufficient clothing, and that they had crowded together in this manner to keep each other warm. It is perhaps in part owing to circumstances of this nature, that epidemics usually break out among prisoners of war in the inclement season.

How far circumstances of this kind might occur at Stapleton I do not know, but undoubtedly the prisoners there had been exposed to the general causes which produce infectious or jail fever. These operating for a time insensibly, gradually enfeeble the constitution, and impair its power of

contending with the disease to which they at last give birth.

I have mentioned that the arrangements of the prison hospital at Stapleton seemed good; that the food, attendance, and medicine, seemed under the circumstances of the case liberal and ample. Yet it cannot be supposed that where an epidemic proceeded so rapidly and extended so widely, such arrangements could have been made before hand in a common prison, where the hospital was fitted up for the occasion, as in an established hospital for the reception of fever; and of this, I could offer from my own observation several proofs. With all these disadvantages, the success attending the mode of treatment at Stapleton, will bear a comparison with the success of the usual practice in similar epidemics, where the collateral advantages have been greatest, and the success most distinguished. And if the success in the last epidemic fever at Stapleton be compared with what has occurred in former epidemics, similarly circumstanced, it will I apprehend appear still more striking.

In this very prison at Stapleton, and in several others, great mortality prevailed among the same description of persons in 1800 and 1801, in part from contagious fever. The details are not known
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to the public, but are no doubt recorded by the Board of Sick and Hurt. In all the prisons of war throughout Europe indeed, epidemics of this nature occur too familiarly. But the records of all these it is feared might be searched in vain, before another instance could be found of eight hundred and fifteen prisoners being seized with jail fever, and only forty-one falling victims to the disease.

That a remedy should have been used by which in nearly one-half of these cases, the progress of the fever should have been arrested in the first stages, is a circumstance I apprehend not merely without parallel, but without similitude in the annals of disease among prisoners of war,

On reviewing the history of the epidemic fever at Stapleton, it may be observed that the contagion was extinguished with considerable difficulty. From the day the fever was first observed, till the appearance of the last case in the hospital, a space of time which I denote the contagious period, three months and ten days elapsed. This may be explained. It was in the ordinary sleeping place of the prisoners, that the febrile virus had its origin. It arose no doubt, from the contamination of the atmosphere, occasioned by too condensed a respiration and perspiration. When contagion originates from this cause, and in this gradual way, it is difficult

difficult to eradicate it from the spot of its birth, because the walls and timbers of the building have become by degrees tainted with it, and retain it long after the healthy degree of ventilation has been restored, and after the moveable articles have been purified by ablution, or exposure to the wind. This difficulty often occurs in the medical history of the navy, when particular ships have been contaminated in this way. These frequently retain the contagious taint for a long while, in spite of every exertion to subdue it.

The contagion at Stapleton would no doubt have been more easily and speedily subdued, had it been possible to have removed the prisoners into other quarters, or to have encamped them in the fields. The first it was impossible to find; and the other expedient could not have been resorted to at that inclement season, even if camp equipment had been at hand, from the general debility and defective clothing of the prisoners, many of whom were affected by pulmonary complaints. These circumstances opposed a proposal that I made, of having all the prisoners still free from contagion bathed in cold water daily, as a means of cleanliness, and a measure of prevention. Had the epidemic occurred in a more southern climate, or in a mild season of the year, much benefit might have been derived from the encampment

ment of the uninfected, and their daily use of the cold bath, as was practised in the 30th regiment.— (See vol. i. chap. iii.)

The benefit deriveable from the cold bath as a preventive, was strikingly illustrated in an epidemic fever which lately prevailed in the Horse Guards, (the Blues) at Canterbury, of which, on this and other accounts, I insert a few particulars, as furnished me by Mr. M'Gregor, lately superintendant surgeon of the Indian army in Egypt, and now surgeon of that regiment. I quote from Mr. M'Gregor's letter to me, dated Windsor, 30th December, 1804.

“ A short account of the regiment previous to the breaking out of the fever will be necessary ; as it will be seen that the modification of typhus which took place was peculiar.

“ The Blues went to Canterbury in October, 1803, in the healthy state which this old corps generally enjoys. In the months of November, December, and January following, a good many cases of fever had appeared, and several died of it ; and I have been informed by my very intelligent assistant, Mr. Peach, that it was from that combination of typhus and pneumonia, which has been denominated pneumonia typhoides. I joined the regiment
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in the beginning of March, there were then few of the men sick, though in April, May, and June, the number was thought considerable for this corps. These were chiefly cases of pneumonia, of slight fever, or an intermixture of the two.

“ In July I first remarked a general appearance of unhealthiness in the regiment, though the total on the sick list only amounted to twenty-eight; yet on the parade it was observed that the men had not a healthy look. Several foul ulcers appeared among them all at once, and any kick or trifling sore speedily degenerated into a foul spreading ulcer; and in several cases extensive mortification took place.” Mr. M’Gregor goes on to state that he was first alarmed with the appearance of contagious fever on the 28th July. A patient was brought to the hospital covered with petechiæ, and died on the 30th. His situation forbade the cold affusion. On examining the barracks at this time, Mr. M’Gregor found them “ extremely crowded and uncomfortable.” On the 11th of August another formidable case of typhus appeared; the patient died on the 14th. Great debility joined to diarrhœa forbade the cold affusion here also. Six other cases speedily followed.

“ In September,” says Mr. M’Gregor, “ our situation became alarming in the extreme, nearly a fifth

fifth of the regiment were on the sick list. In particular there were thirty-three cases of fever, and six of dysentery. On application being made to Sir David Dundas, he ordered the regiment to be encamped, and this was done on the 5th. After this, however, we continued to detect a number of cases of the fever, and the hospital was found too small for the sick. About the 12th, at my earnest recommendation, all the regiment out of hospital were marched three times a day to the river side, and every man was made to bathe. The good effects of this were speedy and manifest, the number of new fever cases decreased daily, and those that did appear wore a milder aspect. Many indeed yielded to the common treatment; in some cases an emetic, and in others the cold bath, at once cut short the disease. We lost no case in October. Indeed it was evident to all, that after the general bathing of the regiment, the contagion stopped; the few cases that occurred after this, were stripped of all the alarming and dangerous symptoms with which the disease broke out. In all, there were sixty cases of fever occurred from July to the 21st October, on which day the last case appeared. We lost six of this number."

Of these fatal cases he details the particulars of three. Two have been mentioned:— in these the cold affusion was not used as a remedy. In the
third

third it was used repeatedly and perseveringly ; and always with apparent benefit : but the case was complicated with diarrhœa, and the patient sunk at last of mere debility, on the 23d day after admission into the hospital. Of the three other fatal cases there are no particulars. " We were successful with the cold affusion," says Mr. M'Gregor, " but it was in such cases as you recommend, and by following your method closely. It did not, however, in general, succeed in cutting short the fever. In one case only did it appear to be followed by bad consequences. In this case dyspnœa and great coldness followed immediately, and afterwards a great determination to the head. The tepid affusion was then applied with success, and though the disease was tedious, the patient recovered. In most cases which occurred, especially where there was a determination to the lungs or intestines, the tepid bath or affusion did the most signal service, and was not unfrequently called for by the men themselves.

" Towards the end of September many cases of dysentery appeared in the regiment ; it soon became epidemic in Canterbury, and proved a most severe and fatal disease to the troops and the inhabitants.

" I have

“ I have mentioned in another place*, that in some suspected corps, the cold bath appeared to have prevented the breaking out of the plague, and to have destroyed the pestilential contagion. I am happy now to find that I have been putting to the test of experiment in Egypt, opinions given out by a learned physician of this country, Dr. Falconer, of Bath †. I am no less happy that experience in the typhus at Canterbury, supports the conjectures of our venerable friend, Dr. Wright, “ that cold bathing will prevent fever, &c. ‡

I wish I could say as much of its preventive power respecting the dysentery of Europe; but from my very limited experience on this point, I will not venture to speak; though the state of the Blues at Canterbury is in favour of such an opinion, only twenty cases of dysentery having occurred after the cold bathing was generally adopted.”

The history of this epidemic suggests a variety of reflections. The Blues marched into Canter-

* Medical Sketches of the Expedition to Egypt from India, by James M'Gregor, A. M. Murray, London, 1804.

† Essay on the Plague by Wm. Falconer, M. D. F. R. S. Bath, 1801.

‡ See pages 237, 238 of this volume.

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bury in good health. There they gradually became unhealthy; while the other troops and the inhabitants of the city enjoyed their usual health.

It is at Canterbury the cause of the unhealthiness of the Blues is to be looked for, and it must have been one to which they were peculiarly exposed, since they alone were for some time affected. On considering every thing, this cause may reasonably be traced to the crowded and uncomfortable state of their barracks; to the gradual operation of foul air, arising from this circumstance, the predisposition to disease is more immediately to be imputed, and finally the contagious fever itself.

The injury to the air of respiration from a cause like this may be various. There is no doubt that it may be in that degree which, continued from day to day, gradually saps the vigour, without for a considerable time producing those symptoms which constitute absolute disease. Illustrations of this may be found in Mr. M'Gregor's narrative. In July the Blues had an unhealthy look on parade, and accidental wounds among them degenerated into foul ulcers. The fever was then impending which in August burst out.

There is no doubt that a fever originating in this way, would assume appearances of malignity from
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the first.—We should naturally expect what really occurred,—great prostration of strength, feeble reaction, early symptoms of putrescence in the fluids, and that tendency to affections of the bowels, with which such symptoms are so generally combined. Cases such as these are less favourable than most others for the cold affusion, which, however, where it had a fair chance, seems to have produced in this fever its usual good effects. But as a preventive its influence was indeed very striking, and deserves the particular attention of military practitioners.

This leads to a subject, which it would be improper to pass over; and on which it would be painful to dwell—the late fatal epidemic fever at Gibraltar. The following particulars respecting it are derived from sources of information on which reliance may be placed.

After a summer, said to have been unusually warm, and a long prevalence of east-winds, during which the town of Gibraltar, which is situated on the west of the rock, was in a great measure becalmed, a malignant fever made its appearance among the inhabitants and troops. It was first observed about the 10th of September, 1804; it spread with fatal rapidity, and continued its ravages with more or less violence

violence till the month of January following; on the 12th day of which it appears by a certificate from the Board of Health to the Lieutenant Governor, that the disease was extinguished. During this period out of about 18,000 inhabitants, military and civil, 12,000 were affected by the fever, and of these about 6000 died.

Unhappily many most important points respecting this uncommonly fatal disease, are differently represented by persons on the spot seemingly of equal authority—these respect its origin, its nature, and its proper treatment.

On the first appearance of this epidemic, the prevailing opinion among the gentlemen of the faculty at Gibraltar, was, that it did not originate in infection or spread by infection, but that it was to be imputed to the extreme heat, or some other general quality of the atmosphere.* Means of prevention founded on the belief of its infectious nature, were in consequence not resorted to for some time; not indeed till it had spread universally. Those who maintained the non-contagious nature of the disease, considered it as highly inflammatory, and resorted to blood-letting and other remedies suited to this idea.

* At one time it was imputed to the effluvia of a burning lime-kiln, which was in consequence extinguished.

By others it is contended that the fever had its origin in imported contagion ; that it was no other than the fever which made its appearance at Cadiz, in August, 1800, and spread to Malaga, Seville, Xeres, and the surrounding country, where it was said to have carried off 100,000 persons. The contagion seemed never since that time to have been wholly extinct. The fever is said to have re-appeared with great violence at Malaga, in August, 1803, when the Spanish government, to prevent its spreading, established a cordon of troops round the town, which on this occasion is supposed to have lost 17,000 souls.

In August, 1804, it appeared again at Malaga, a month previous to the epidemic's breaking out at Gibraltar. The inhabitants of Malaga fled on this occasion on the first alarm, (probably to avoid being confined by a cordon of troops as before) to all the towns along the coast from Cadiz to Carthage, carrying the disease along with them; and hence the fever, which was more general this year than at any former period, is supposed to have found its way into Gibraltar.

This representation is strengthened by the following circumstance. In former years when the fever prevailed at Malaga and elsewhere, measures of precaution were adopted at Gibraltar to prevent its introduction there; particularly that of a quarantine on all vessels from Spain, while vessels from the

the ports where fever was supposed to prevail were not allowed even to anchor. Health-guards were established at all the landing-places, and guard-boats were stationed in different parts of the bay. The medical practitioners, the catholic priests, and the heads of the Jews, were directed to report every suspicious case of fever, especially among smugglers and other strangers, who might clandestinely gain admission into the garrison; and in some instances such cases were removed to the lazaretto with the persons who had harboured them; and the houses in which they had been found were burnt to the ground. These measures, which in former years had apparently proved so salutary, had been wholly neglected in 1804, and when the fatal epidemic actually appeared in Gibraltar, it was rashly concluded that it had not been received by infection, and was not in its nature infectious, so that measures for preventing its spreading were for a considerable time wholly neglected. When they were had recourse to, being adopted in the midst of terror, confusion and death, they were imperfect, variable, and unsuccessful.—

We now know that the fever *was* contagious*;

* I use the words *contagious* and *infectious*, as synonymous. The Anglo-American distinctions in the use of these terms, will only be adopted by those who adopt the dangerous doctrines which gave rise to them.

evidence has accumulated on that point to convince the most sceptical. And this being admitted, the preceding account of its origin is consistent and probable.

Having examined several reported cases of this fever and the accounts of some dissections, the following seems to me a general summary of the symptoms. After some general uneasiness and sense of cold, the patient is seized with headach or vertigo, pain in the back and limbs, and great sense of languor and debility. The pulse is frequent but not strong; the skin dry; the tongue lightly furred; the thirst and heat preternaturally great, but not violent. These symptoms increase; nausea comes on, and vomiting often ensues. Debility augments rapidly, and the stomach becomes more and more irritable. Dullness, stupor and low delirium come on; the body is covered with petechiæ. With an almost constant disposition to nausea and vomiting, there is also a tendency to purging.—The sensibility decays more and more—the patient utters no complaint—the abdomen swells—blood is discharged in stools, and sometimes from the nostrils.—The pulse sinks; the extremities become cold; and the patient expires quietly. On opening the stomach and bowels, they are found in a state of commencing mortification.

In the course of the disease, many complained of great anxiety and oppression about the præcordia,
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with a small and fluttering pulse. Many had the black vomit, and some had a retention of urine, or rather a deficiency in the urinary secretion. These were very unfavourable symptoms. In many cases mortifications of the extremities took place—in some few, anthrax and parotis. The disease ran its course rapidly—many expired on the 4th and 5th day, and some earlier.

These are the usual symptoms of pestilential fever.—They differ little from those of the plague, except in the absence of bubo, to which however there appears to have been a tendency. It is to be lamented that we have no accurate account of the heat in this disease, the thermometer not having been applied, so far as is known, in a single instance.

From the accounts of the treatment which I have seen, it was usual with those who did not conceive the disease to be inflammatory, to administer emetics in the first instance; afterwards gentle purgatives—salines—opiates—mineral acids—and wine in moderate quantities:—occasionally also æther and other cordials. Blisters were commonly applied. Bark does not seem to have been generally administered, the state of the stomach scarcely admitting it.

On the whole the method of treatment, however different in the hands of different practitioners,

seems

seems to have been very unsuccessful. Of all that were seized with the fever, one half are said to have perished, a proportion greater, I believe, than is stated in the well-authenticated history of any modern epidemic fever, and nearly equal to what has occurred in the plague itself.*

Neither were the means of prevention, too tardily adopted, to appearance more efficacious.—The epidemic raged four months. It lessened in December, and died away in January, but whether in consequence of measures of prevention, or of the progress of the cold weather, or of other causes, does not appear. The fever of Malaga usually disappeared in December.

Those specific contagions of which the constitution is indefinitely susceptible, seldom make two attacks in immediate succession. Were it otherwise their ravages would know no bounds. Hence it happens that after a time, they naturally stop for want of victims. At Gibraltar two thirds of the whole population were affected by the fever before it ceased, as great a proportion probably as has been seized at any time by an epidemic fever.

* On a comparison of the accounts of different authors, Dr. Falconer found the deaths in the plague to be to the recoveries nearly in the proportion of 10 to 9.—*Essay on the plague*, by W. Falconer M. D. F. R. S. p. 19.

It has been said that such of the officers and soldiers as had before been affected by the yellow fever of the West Indies, escaped the fever of Gibraltar. Such assertions cannot be admitted without strong evidence. The yellow fever of the West Indies, if we may credit the most respectable testimony, has often affected the same person twice; and from the latest and most indisputable accounts, the plague, to which the fever of Gibraltar, bore so much resemblance, though seldom repeating its attacks without some interval of time, may however be received by the same person indefinitely.

The reader will naturally expect to hear some account of the effects of the cold bath, or cold affusion in the epidemic of Gibraltar, as a remedy and a prophylactic. Considering that this epidemic held its fatal career for four months and upwards, and that all the usual modes of treatment failed, it might naturally have been expected, that those in attendance on the sick and dying, would in these desperate circumstances have had recourse to a mode of practice, which in similar situations had been attended with uncommon success. The first volume of the Medical Reports, had been seven years before the public, and had passed through three editions in our own language. The second volume had also appeared, and had met general attention. The work had been favourably received on the Continent, and began to influence

fluence the military and naval practice of foreign nations. Of this a remarkable proof had appeared in the second volume, in the narrative of the physician Dr. B. A. Gomez, who in the summer of 1802, when a fatal fever prevailed on board the Portuguese fleet, had recourse to the cold affusion after every other remedy had failed, with a success altogether extraordinary. This was the more striking as the fever so far as we can judge, seems to have been the same that committed so much devastation in various parts of the Spanish peninsula, and that at length intruded into Gibraltar itself:—the more striking also as the fever in the Portuguese fleet occurred in the straits of Gibraltar, in sight of the garrison, and the extraordinary success of Dr. Gomez, even had it not been published to the world, might have transpired to the practitioners on shore. It is a duty, however, that I owe to the public to declare, that the fatality of the fever at Gibraltar, brings no imputation on the mode of treatment recommended in these volumes, for after the most diligent enquiry, I cannot find that the cold bath in any form was used either as a prophylactic or a remedy in any single case of the disease. Neither can I find that tepid ablution or affusion, was in any case resorted to, or that water was recommended as a drink. I record these facts with feelings very different from those of wounded vanity—they bear hard on the feelings of any other person, regret the circumstance.—But this is a case in

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which

which my sense of duty obliges me to speak out. I will however make no comment, but leave the subject to the reader's reflections !*

On a review of the fatal epidemic at Gibraltar, there are other circumstances to excite our deep regret. Of this kind is the neglect of the usual means employed to prevent the introduction of fever
into

* In a letter to Mr. M'Gregor from Mr. Bennion garrison surgeon of Gibraltar, a copy of which has been communicated to me by the kindness of Dr. Batty since the above was written, I have found a very clear and I dare say a very exact description of the fever, which as it is to appear in the Medical and Physical Journal for August, I decline to insert here. It agrees in substance with the account I have given, and shews the good effects of the warm ablution.

“ At first, ” says Mr. Bennion, “ it appeared to be the opinion of the majority (of the medical men in the garrison,) “ that the disease was not contagious, a fatal opinion to be “ acted on, as subsequently appeared. I arrived here a few “ days after the disease made its appearance, and of course “ could not give an opinion on the point, till I had enquired “ into its history and symptoms. Alas ! I soon found it to be “ the nearest relation to the plague, some cases from the first “ putting on the appearance of that disease, often of the typhus gravior, and others of the yellow fever.”

“ As to the mode of treatment, that which I followed is “ very simple and soon described. I have little to remark on “ the plans of others : it is known however that they were not “ equally successful. Some of our ablest men were inclined
“ to

into that garrison, when the ports of Spain were affected by contagious fever. Of this kind also was the

“to interrupt the disease but little, and gave little but diluents
 “and cordials. Others bled very freely; and others gave the
 “bark liberally and early.”

“My mode of treatment was as follows. My first step
 “was invariably to put every patient into a bath which was ge-
 “nerally warm. When taken out the body was well rubbed with
 “soaped flannel, and then he was put to bed. If the powers
 “of life were strong, an emetic solution was next given of
 “tartar, antimonial, and natron vitriolatum. The solution
 “generally operated pretty smartly both on the stomach and
 “bowels, and when it did its office well I frequently had little
 “more to do but remove debility. The patient being often
 “well on the third day. If the solution persevered in for
 “some time did not operate, which was frequently the case at
 “first, the stomach and bowels being very insensible, I gave
 “calomel, and continued giving it either by itself or with jalap,
 “or with the compound extract of colocynth. I endeavoured
 “by all means to keep up the alvine discharge when obtained,
 “(till) the patient was found perfectly relieved, and free of
 “fever. If not, the 4th or 5th day generally put an end to
 “all enquiry. But in many no evacuation could be procured
 “by any means, in others there was a violent diarrhœa.

“I dissected the bodies of a few. The general appearances
 “were the destruction of the internal coat of the stomach,
 “inflammation and ulceration of the intestines, and some-
 “times of the (other) abdominal viscera. I have little more
 “to say than that after procuring evacuation, I pursued saline
 “medicines, when little fever remained, but when the disease
 “continued after the third day, it frequently turned out to be

the rash declaration, when the fever did appear in the garrison, that it had not originated in contagion and was not in its nature contagious : two propositions distinct in themselves, but which have been presumed without proof to involve each other. However general the contrary opinion may be, the fever of Gibraltar, so far as we know, might have its origin in some noxious quality of the general atmosphere, or in marsh miasmata, and yet be propagated in the second instance by infection. It is greatly therefore to be lamented that measures were not taken immediately to separate the sick from the healthy. These ought indeed to be resorted to in every instance of the appearance of general fever, especially as according to the present state of the evidence, they are equally easy and effectual. There is no safety in considering a fever to be non-contagious in the first instance, and waiting for evidence to the contrary. There is neither wisdom nor mercy,

“ the severest typhus. I then found the greatest benefit by
 “ Dr. C. Smyth’s method, as laid down in his book on the Win-
 “ chester fever. Opium or bark did not succeed : when libe-
 “ rally given I perceived them doing mischief.”

This account of Mr. Bennion’s practice shews the benefit of early ablution, and of early evacuations, which though with a considerable waste of strength as conducted by him, must have taken off the febrile heat, and in some cases cut short the disease. I was in hopes from the general accounts I had received, of finding some particulars of the effects of the cold bath in his practice, but have been disappointed.

declaring

declaring it to be non-contagious while the point is in doubt, with the view of preventing general alarm. To have adopted the measures of precaution requisite in the case of an infectious epidemic, unnecessarily, is a light evil. In a military garrison, where subordination and discipline are already established, it is no evil at all. At the worst, some trouble may have been incurred which might have been spared. —How melancholy the reverse of this supposition! What shall the theorist say for having trusted for safety to his speculations, where the lives of thousands were at stake? what reparation can he make for his errors, when the proofs that convict him, are the graves of his countrymen? To pronounce a disease to be contagious, ought not to deprive the sufferers of the aids of science or of humanity, as some weakly suppose. It ought now to be generally known that simple means of precaution, *adopted early and strictly adhered to*, do away the danger of the attendants, and the practitioner of medicine cannot trust his own safety to these, is unworthy of his office, and ought to lay it down. And, let it never be forgotten, that the means of prevention to the un-infected are means of cure to the sick. To keep off the idle and unnecessary intrusion of those in health, is to secure to the sick silence and quiet;—to enforce universal cleanliness and ventilation, is to increase their comfort, and support their strength; while the regular and prudent use of personal ablution, which of all the means of precaution

precaution is perhaps the most important to the healthy, is of all the means of cure certainly the most efficacious to the diseased.

The practice I have recommended in fever is becoming general in the navy, of which a variety of proofs have been furnished me. It is also spreading in the army, and it has attracted the notice of his Royal Highness the Commander in Chief.

The official reports of a fever in the Sussex Militia, in barracks at Chelmsford in January last, have been communicated to me by the kindness of Mr. Knight, Inspector General of Hospitals. In these Mr. Knight and Dr. Roberts bear strong testimonies to the efficacy of the cold affusion, both as a remedy and a prophylactic; and the former has earnestly recommended to his Royal Highness the Commander in Chief, the equipment of every barrack hospital with a shower-bath, the slipper-bath with which some of them are supplied, being applicable to warm bathing only. This recommendation has met with due attention, and is now carrying into effect. There is reason to believe also, that our military practitioners will in future be furnished with thermometers, for ascertaining the heat in febrile diseases, of which we may in future expect more accurate reports.

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The state of my health having obliged me to abandon my laborious duties at Liverpool, I passed the winter at Clifton, as was before mentioned, and fixed my residence in Bath in the present spring. Typhus fever, is comparatively speaking, of rare occurrence in this city. It has however been my good fortune to prescribe the cold and tepid affusion in several cases of typhus, and of scarlatina, in Bath and at Clifton, with its usual good effects. There seems no doubt of this method of cure extending here and every where. My task then I hope is finished; and with a few words more strictly of a personal nature, I shall lay down my pen.

Having had an apparently hazardous, but in my judgment a highly salutary medical practice to recommend to the world,—a practice contradictory to long established and almost universal prejudices, I relected beforehand with the utmost seriousness on the duty imposed upon me, to avoid in my manner of presenting it all possible grounds of offence. If my matter was alarming, if my object was bold, I have endeavoured to make my manner calm and temperate. The claims of my contemporaries to merit on this occasion, so far as I was acquainted with them, I have studiously brought forward. I have been desirous of treating them not merely with justice but generosity; and many series of experiments which I myself have undertaken, and I may say undergone, especially in investigating the effects of perspiration on animal heat, I have suppressed

in the detail, and only given in the result. In a word, it has been my endeavour to suppress all personal considerations, and all petulant expressions; where I could employ the authority of others, to do it freely and respectfully; and where I have been led by my subject to controvert opinions before the world, to use the language of civility and candour.

By these means I have endeavoured to disarm personal opposition, and to avoid controversy—controversy which some philosophers have invoked, but I think unwisely; and which on a science so imperfect, so important and so difficult as that of medicine, seems to me to have almost uniformly involved consequences of an injurious and melancholy nature.

On the whole my endeavours have been successful. I have encountered little opposition; I know not that I have provoked any man's enmity; while the medical writings of the day, both in Britain and in America, bear evidence that considerable changes have been effected and are effecting on the opinions and conduct of medical men, quietly and insensibly, on points of no mean importance, in physiology, as well as practice.

J. J.

Bath, 6th July, 1805.

APPENDIX.

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Preface to the Letter to Dr. Clark:

A CONTROVERSY having arisen among the gentlemen of the faculty at Newcastle, as to the safety of admitting patients under fever into certain wards of the Infirmary there, separated in every other respect from the rest of the hospital, but under the same roof, my friend, Dr. Clark, appealed to the opinions of his medical friends and correspondents, in different parts of the kingdom, and collected a body of evidence in support of the measure, which seems altogether decisive. The whole collection* contains perhaps the clearest and most satisfactory information on the question respecting contagion, that is any where on record. The following letter forms a part of that collection; as it contains some curious facts on this important subject, it is inserted here.

* Published under the following title, "A collection of papers, intended to promote an institution for the cure and prevention of infectious fevers in Newcastle, and other populous towns. Together with the communications of the most eminent physicians, relative to the safety and importance of annexing Fever-Wards to the Newcastle and other Infirmaries. By John Clark, M. D." Two vols. 12mo. Newcastle, printed by S. Hodgson, 1802.

APPENDIX.

No. I.

Letter to Dr. Clark.

Dear Sir,

Liverpool, August 5, 1802.

TO the great mass of evidence and authorities which is already adduced in favour of the plan of receiving fevers into the new building attached to your Infirmary, I can scarcely consider any addition as necessary ; but as the experience derived from our fever-wards here is now of fifteen years duration, and as it may bear more or less on the important question at present agitated with you, I am happy to comply with your desire, in giving you a more full account of it.

In the report of your committee is republished an extract of a letter of mine to Dr. Percival, dated May, 1796. I there mention, that for five

years and an half, we had received patients in fever into the Liverpool Infirmary, and for the greater part of the time, into two small wards on the ground floor of the left wing. These patients were on the general establishment of the charity, and had their food and medicines provided in common with the other patients. They entered into the yard of the wing by the same large folding doors which admit all the other patients; but the fever cases were afterwards conveyed into these two wards directly, without using the common stair-case of the wing. Immediately over them, however, were the wards of the other patients, and it was impossible to seclude the nurses of the fever-wards entirely from the other servants of the house; yet, in no single instance was the contagion extended to the contiguous wards. I have also mentioned, that at the end of five years and an half, the place for the reception of fever was removed from those small and ill-constructed wards, to two large and airy wards in the centre of the work-house, a description of which is given. Four years had then elapsed from the commencement of that arrangement, during which time there was no reason to believe that the contagion had, in any instance, spread from the fever wards to the rest of the building. Six years more have since elapsed, and, on the strictest inquiry, I find,

that the same assertion may be made up to the present day.

In order to point out the nature and value of this experience, I must be more precise and minute than is agreeable, and must even repeat some particulars in my former letter.

Our work-house is a very large building, which sometimes has contained one thousand four hundred persons, and which is in many respects very imperfectly constructed. The great door in the centre of the building opens immediately into the great dining-room, but is never used. All the persons who enter the house, sick or well, pass through a small door in the right wing, seven feet high, by about three feet and a half in width, where a porter constantly stands to prevent the exit or entrance of the inhabitants, excepting under the rules of the house. The sick are received at this door, whether labouring under fever or not, and are carried across a passage to a receiving house, of which there is one for each sex. Here they are stripped and washed, and their clothes changed; the apothecary examines them on his daily visit, and sends those under fever to the fever wards. The access to these wards, is, as I formerly mentioned, through the common stair-case of the centre of the building. On the ground floor is the great dining-

dining-room, the access to which, from the rear, is through the area at the foot of this stair-case. On the next floor, is the lock hospital for females; on the third floor, the fever-wards; and on the fourth, the children's nursery.

For the ten years in which the wards have remained in this situation, there has been no contagious fever in the lock hospital. But, two years ago, a very alarming fever broke out in the nursery, and extended to no less than 67 children, all of whom recovered by the early application of the usual remedies, of which early and frequent ablutions formed the principal part. This might be supposed to have originated from the fever-wards below—but, on enquiry, it was clearly traced to another source, as you will see by the letter from Dr. Bostock, the attending physician at the time, a man of great accuracy.* Except

* About two years ago, a very alarming fever broke out in the nursery, by which no less than 67 children were infected. The source of this disease was, however, clearly discovered; a family, residing in a cellar in one of the most confined parts of the town, was sent in a state of fever to the work-house; the parents were placed in the wards, but by some neglect, the children were sent into the nursery, with some degree of the disease upon them, and without removing the infected clothes which they had worn before they came into the house.

Extract of a letter from Dr. Bostock, to Dr. Currie, printed in vol. i. p. 199, of Dr. Clark's Collection.

in this instance, contagious fever has been unknown in the nursery, where the apartments are clean, spacious, and well-ventilated, being at the top of the building, and the children have the appearance of health and vivacity.

Whatever your sentiments may be respecting the narrowness of the sphere of contagion, I think you would not have ventured to predict such a singular exemption from fever, under such circumstances. For the children are continually passing up and down the stairs, and playing in the stair-case; and, at particular times of the day that is, immediately before dinner, they are crowded towards the bottom in a singular manner. The fact is, their food is distributed to them from the dining room, where a great body of the people from the wings of the house dine; and as the doors open at a certain hour, a great crowd collects previously at the bottom of the stair-case, and in the area below, struggling for admission. While I was examining this ill-constructed stair-case, which is never clean or sweet, about two years ago, a patient, under fever, was brought across the court, a little previous to the dining hour. I followed the patient up into the ward, and counted exactly eighty-three children on the stairs, within three or four feet of every one of which, the patient must have passed, and
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to some much nearer. On mentioning the circumstance to the nurses, they seemed to think it nothing uncommon, but as what might happen any day. Though no clear instance of injury arising from such occurrences can be brought, I have always deprecated the circumstance which leads to them, and in conjunction with others of my brethren, urged, in the parish committee, the propriety of having a distinct passage to and from the fever-wards. Various consultations were held on this point, but the structure of the building rendering such an alteration impossible, without entirely defacing the front, the want of any actual proof, or even appearance of injury from using the common stair-case, cooled our zeal. The probability of a separate house for fever, from the inadequacy of the present wards, rendered it less necessary to press the alteration, and the proposal is at length happily superseded, by the actual erection of such a house, now in rapid progress.

The fever-wards themselves, though very far from being sufficiently extensive, are admirably ventilated, and, on the whole, very happily conducted. The nurses still live in these wards, night and day, their apartment being in the centre between them, and open at the top to the air of both, as described in my letter to Dr. Percival,

cival, already referred to. In the course of ten years, several of them have caught the contagion, but assistance being immediately had, one only has died, so far as I can learn, a woman upwards of sixty, and otherwise very infirm.—In the instances where the contagion has been thus communicated, it has been to nurses newly introduced. They have seldom been, any of them, affected more than once; their constitutions acquiring, by habit, insensibility to the contagious impressions. I mentioned to you, that two of the present nurses have each of them a child actually living with them in the wards, and going out to school in the day. These children appear neat, clean and healthy, though they sleep in the very centre of the patients every night. Their mothers were convinced that they were not liable to any injury, for they never came into contact with the patients, and they seemed to think they should be themselves perfectly safe, if it were not that they are employed in offices that oblige them to be often, and sometimes for a considerable time together, in contact with the sick, and exposed to the undiluted exhalations from their skin, and their lungs.

Experience has, however, taught them to estimate even this hazard very lightly; and it is, in fact, as easy to get a nurse for the fever-wards,

as a servant for any other part of the house. This exemption of the nurses from contagion, (a few instances excepted) is to be attributed not merely to the ventilation of the wards, but to the singular cleanliness of the patients, on every one of whom, in whatsoever stage of the disease, complete ablution is performed in one form or other, at least once every day; this being done where the fever is high, and the heat considerable, with water perfectly cold; and where the strength and heat are reduced, with water tepid, or even warm, and sometimes mixed with vinegar or sea salt. The methods of Morveau or Carmichael Smyth, have never been practised in these wards, or in the former wards of the Infirmary; and our experience seems to decide, that the proper use of pure water and pure air, may wholly supersede them.

To the practice of completely washing and changing the patients in the receiving houses, before they are carried up the great stair-case, I attribute the extraordinary circumstance of the contagion never appearing to be communicated to the bye-standers in this narrow passage, of which a melancholy and striking circumstance has impressed conviction on my mind. Notwithstanding the healthfulness, and especially the exemption from fever, of all the places in the immediate vi-

cinity of the fever-wards, in the year 1801, the master and mistress of the house, and a young woman, daughter of the assistant mistress, were at different times affected with typhus, and all of them died. This circumstance occasioned great agitation. The master of the house had never been in the fever-wards, the mistress very seldom; and they lived in a part of the building very remote. But Miss Nickson, the young woman alluded to, had, it was found on inquiry, been incautiously turning over the linen from the fever-wards before it had been steeped in water, and to this circumstance her fever was imputed. In all these cases (one of which only I attended) the disease proceeded insidiously, and was scarcely suspected till it was too late. But how, you will ask, does the case of the master and mistress apply to the opinion I have given on the benefit of ablution in preventing contagion? I have before mentioned, that all persons, sick or well, and the patients under fever among others, enter the work-house by a small gate in the right wing. This gate is close by the apartments of the master and mistress, that they may have this important pass immediately under their eye. In fact, the window of their parlour is on the outside of the gate, and within the distance of four feet, and the door of a coach, bringing up a patient under fever, must, from the narrowness of the passage,

within a foot or two of this window. Here the patient is taken out and carried through the door, and generally examined by the master or mistress in the inside. In the year 1801, in this great town, there were admitted on the books of the Dispensary, nearly six thousand patients beyond the usual number; from twelve thousand, they increased to eighteen thousand, and typhus fever was extremely prevalent. Of course the distress of the poor was great; there was an unusual pressure for admittance into the fever-wards, which were filled beyond all former precedent, and many were obliged to be let away. The examination of these unhappy persons in their unclean and contagious state, unflushed and unventilated, and the rejection of them when necessary, devolved on the master and mistress, attentive and humane persons, who doubtless fell victims to this dangerous and painful duty. This is the opinion of their successors, Mr. and Mrs. Hall, who assure me, that they make a point of never approaching within a yard of two of suspected fevers, and have hitherto escaped. The porter, who has opened and shut the gate for several years, has escaped also. He assures me, that he has used the same precaution; he inclines to ascribe much of his safety to the use of tobacco, of which he chews very large quantities. The persons employed in washing
and

and cleansing the clothes of the patients, in the receiving houses, have sometimes been affected by fever; but being on their guard, and applying for assistance in time, fatal consequences have, I believe, never ensued.

The striking advantages of external ablution, both as a preventive and a remedy, have engaged our particular attention in the construction of our new fever hospital. Before the patients enter into the body of the building, they pass by an entrance peculiar to themselves, into a vestibule, where there are baths of every kind, where they are stripped of their foul clothing, washed, and clothed in the hospital dress, and thus purified and refreshed, removed into their proper apartments. In the plan which you had the goodness to show me, I believe there is a provision of the same nature; and if not, I would submit the propriety of superadding something of the kind, not for the safety of the patients in the adjacent wards of the Infirmary, but for the benefit of the fever patients themselves, and their immediate attendants. For as to any danger to the patients of the Infirmary, from the mere proximity of the walls of the fever-house, while you enter by the distinct passages, and observe the usual and obvious precautions, the details which I have given you will show you, that it is impossible for me to listen

it for a moment, even if the uniform experience of the Physicians of Liverpool were not supported by that of every other part of the island, where it can fairly be collected. Nothing, indeed, seems to be more firmly established, than the narrow sphere of even the most virulent contagions, where the air is allowed to circulate freely. Hence, in the torrid zone, where the heat of the atmosphere in a manner forces ventilation, the infectious quality of the most malignant fevers is a matter of dispute among the faculty, though I believe with yourself and Dr. Wright, (the present worthy President of the College of Physicians at Edinburgh) that, under a similar deficiency of ventilation, they would be equally, or more infectious, than the fevers of our northern latitude. Dangers that cannot be calculated, are always magnified by the imagination; and the baleful influence derived to the atmosphere, from taking its constant course over extensive swamps of many thousand acres of putrefying animal and vegetable matter, has been loosely applied to the same air, passing over a human body, for a thousandth part of the time, and a millionth part of the surface. Yet the testimony of all actual observers, in every region of the earth, is calculated to correct this error. Even the sphere of the contagion of the plague, the most terrible of the diseases which affect the human species, seems limited to a very few

few feet, or even inches, in a free circulation of the air ; and it might be received into your projected fever-ward with safety to the patients of the Infirmary, if we may believe the concurrent testimony of Savary, Bruce, Russell, and of Antes, the most recent and satisfactory of them all.

It is folly to pretend that this subject is of a professional nature, and not cognizable by any fair understanding. The facts are numerous---the inference easy. It is only necessary for unprejudiced men to make themselves masters of the first ; the last, seems to me inevitable. Even those who will not take the trouble of obtaining the information necessary to inform their own judgments, might regulate their conduct safely, by conforming to the usual maxim on similar occasions—that of adhering to the opinion of those who are likely to be best informed. That persons, the business of whose lives is to observe and to combat the effects of contagion, should be best acquainted with its laws, and their evidence best entitled to weight, in a question depending on those laws, are propositions that no one will dispute ; and the force of which can only be eluded by shewing that, in the point at issue, they have an interest likely to pervert their judgment or their evidence. But what peculiar interest have the faculty in institutions for the prevention of disease, unless,

less, indeed, the honourable reputation they may derive from serving the community? What interest have members of the faculty, already in the possession of public confidence, in committing their reputation to hazard, in the support of plans of a dangerous nature? If such plans are carried into effect, their effects cannot be controlled; and if they turn out to be such as their opposers prophecy, they must destroy the reputation of their supporters.

In one point of view it is perhaps fortunate for the world that the controversy at Newcastle proceeds to such a length. The subject will undergo a complete investigation, and the combat you are maintaining will in the end, I have little doubt, decide the question, not for Northumberland only, but for every part of the kingdom where it still remains undecided. That much interest will attach to your proceedings, not at the present moment merely, but in future times, entertain little doubt. In this, and in every view of the subject, I have great pleasure in ranging myself on your side, and in staking, with confidence, what little character I have upon the issue.

With every sentiment of respect and regard,

I am, Dear Sir,

Your faithful friend and servant,

J. CURRIE.

The following Letters on the Plan for erecting a LUNATIC ASYLUM, at LIVERPOOL, having been several times applied for by persons engaged in similar undertakings, are now given to the public at large.

Liverpool, August 29th, 1789.

Mr. Gore, Printer of the Liverpool Advertiser,

Sir,

Be so kind as to give a place in your useful paper to the following remarks on public charities, and particularly on the proposal for a Lunatic Asylum.

In forming an idea of the connection between the various ranks of society, we may consider a nation as a great trading company, and if we suppose this company to be engaged in both manufactures and commerce, these terms will, in one sense or other, apply to almost all the occupations of civilized life. Each partner in the business is not equally *concerned*, it is true, because every one does not throw the same share of property

perty, talents, and activity, into the common stock, but all are interested in the general success, and the welfare of each is connected with that of the whole.

A business of this kind naturally divides itself into various branches, in which the different partners must engage according to their respective knowledge and abilities. That there may be regularity and order, there must be a proper subordination; each must exert himself honestly in his particular department; and while some plan and regulate, others must labour and execute.

Of these two divisions, the last indeed is by far the most numerous. A few suffice to give general directions, but many are required for the manual operations. The manufactures carried on are almost all of them from raw materials, and demand much time and labour to bring them to perfection, and the exchange of these manufactures, with those of other *great trading companies*, is business of great enterprize and exertion. The earth is hard and stubborn, the ocean is dark and tempestuous. To conquer the ruggedness of the one, and to triumph over the dangers of the other, the great bulk of mankind must work and toil. It is indeed evident, that the various classes of men grow more and more numerous the lower they descend.

A well

A well regulated society may be compared for its solidity to a pyramid. It may be compared to a pyramid likewise because it terminates in a point, because the strata of the building are of greater circumference as they approach the ground, because each inferior stratum, supports all that are above it, and because the lowest stratum, which is the widest, sustains the building: here the analogy fails. The foundation of a pyramid becomes more solid, the greater the superincumbent weight. It only sinks the deeper in the earth. But the foundation of this living edifice is made of less firm materials, and if it be too much pressed upon, it will crumble away.

This comparison may serve to illustrate the immense consequence of the labouring poor. They demand our constant attention. To inform their minds, to repress their vices, to assist their labours, to invigorate their activity, and to improve their comforts:—these are the noblest offices of enlightened minds in superior stations; offices which are of the very essence of virtue and patriotism, which must attract the approbation of the good and wise, and which will obtain the favour of the Eternal Being, who is the Great Father of us all.

But of all the claims which the poor have upon us, there is none so pressing, or so generally admitted, as that for assistance when sinking under

Vol II. X disease

disease. Accordingly, institutions for the relief of the sick poor have been established all over the kingdom, and are perhaps the most unexceptionable of all public endowments. In this great and encreasing town, institutions of this kind have been supported with singular liberality, and have been attended with singular success. One disease only has no provision for it; one disease, awful in its appearance, and destructive in its influence, but of so peculiar a nature as not to admit of relief under any general establishment. It is needless to say that this is Insanity. The difficulty and expence of founding Asylums for Lunatics, have been the only reasons, it may be presumed, why they have not been universal, since it cannot be doubted, that they have the same general recommendations as hospitals for the sick, and that they even have peculiar claims in their favour, both of policy and of humanity.

Of the various evils to which men are subject, there is indeed none so dreadful as insanity. Other calamities are exterior, and pass away with the flight of time; or if they are mental, they yield to the constant succession of external impressions. If human nature is unable to throw off other evils, it happily sinks under them, and death presents itself to the good and the brave, as the termination of calamity. But madness, while
it

it hastens not the approach of death, destroys all that makes life valuable. It is not a single enjoyment, of which it bereaves us, nor a single blessing that it carries away. It preys not on the gifts of fortune, but on the attributes of reason, and strikes at once at all the powers and privileges of man !

Yet if the victims of this fearful malady were incapable of relief, as some rashly imagine, we should have only to tremble at their fate, and to mourn over the degradation of our nature. But while experience teaches us that their situation is by no means hopeless, as men and as Christians, we are called on to exert ourselves in their behalf. If affliction of any kind engage our attention, if disease in any form excite our compassion, let not those be denied our pity and our succour, whose affliction is the most deep, and whose disease is the most terrible.

In the institution of a Lunatic Asylum there is this singularity, that the interests of the rich and poor are equally and immediately united. Under other diseases the rich may have every assistance at their own homes, but under insanity, relief can seldom be obtained but from an establishment for the treatment of this particular disease. Hence the objects of a Lunatic Asylum, are two-fold—to provide accommodations for the poor suitable to

their circumstances, and to make provision for those of superior stations, who are able to remunerate the expence. The objects of such an institution are two-fold in another sense: It holds out a shelter both for the curable and incurable. To the first it proposes the restoration of reason, and while it relieves society from the burthen of the last, it covers the hapless victims themselves from the dangers of life, and from the selfish contempt of an unfeeling world.

A Lunatic Asylum differs from hospitals for the sick in another important particular. These require not only a great expence for their original establishment, but a great annual contribution for carrying them on, since the patients in hospitals have not only their lodging and attendance gratuitous, but their food also, and sometimes their clothing. On the contrary, in a Lunatic Asylum, the expence of diet and clothing (except in very particular cases) never falls on the institution; this being defrayed for the paupers, by the parishes to which they belong, and for those in better circumstances, by the guardians of their property. It is the policy of an asylum to make these two classes connect with each other, so that the increased payments made by the rich, may serve to diminish in some degree, the demands on the poor. Hence the annual expence of an Asylum is small

small, compared with that of hospitals, properly so called, though the expenditure required for the erection and fitting up of the building, must no doubt be considerable. But it is not the character of the inhabitants of Liverpool to let a plan for a public institution that can be proved to be useful or honourable, fall to the ground, for want of contributions to carry it on; much less, a scheme of humanity so interesting and important. Let the usefulness of this scheme be made apparent to the public, and its success is infallible.

Every informed mind must indeed rejoice, that the general meeting called at the Infirmary to consider of the propriety of an Asylum for Lunatics, were unanimous in their approbation of the measure, and that a committee appointed by that meeting, are now preparing a plan for carrying it into effect. If the funds for the Asylum can be raised, without infringing on the interest or property of the Infirmary, the friends of that hospital will doubtless rejoice to see the institutions connected together, by which mutual advantages may be obtained, and the great object of all such charities, the relief of human misery, be promoted and extended.

The only other point to be considered, is the extent of this Asylum. The most prudent con-

duct will be, not to enlarge it much beyond the present necessity, but to erect it on a plan, which may admit of future additions, according as future experience may point out that they are required.

Under these restrictions it is to be hoped that the public voice will be unanimous in favour of the proposed establishment; and that the magistrates especially, will lend it their support. So shall another evidence be reared, in addition to those which already reflect credit on the munificence of Liverpool. Our public buildings for pleasure, as well as business, are in a high stile of elegance and splendour: our institutions for the care of man's perishing body are already perhaps brought near to perfection;—our honours will be increased, and the system of our charities completed, by an institution for the health of his immortal mind,

J. C.

October

October 15th, 1789.

Mr. Gore,

Sir,

IT gives me pleasure to find, that my letter to you of the 25th of August, on the proposal for a Lunatic Asylum, has been so far noticed, that its defects have been discovered, and that some gentlemen are desirous of seeing them supplied. I proceed therefore willingly to discuss those points which connect the subject with our particular situation, and which the limits of your paper prevented from being included in my first address—It may seem, indeed, that as the measure has been approved at a general meeting called by the mayor, where the scheme proposed by the committee was examined and adopted, it is not now necessary to enter farther into the business. But as objections have been started from some respectable quarters, and as several gentlemen of property and character seem as yet to hesitate on the propriety of the measure, it may not be improper to consider it more particularly.

1. It is asked how our Lunatics have been hi-

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therto bestowed, and what inconveniences have been felt from the want of an Asylum.

Hitherto such as have not been sent to a distance, have been confined in the Poor-house, a building erected for the reception of helpless infancy and of declining age, which contains within its walls upwards of a thousand objects of this description, and which neither has, nor can be supposed to have, proper accommodation for Lunatics, who require so very peculiar a treatment. Hence the burthening of the Poor-house with the insane, has been attended by many serious inconveniences. It has introduced disquiet and disorder into the institution, when the Lunatics have been suffered, as is common, to run at large; and where they have been placed in confinement, no adequate provision or attendance having been provided for it, unhappy consequences have followed to these hapless beings, over some of which humanity laments, and delicacy must draw a veil. In such a state of things, much could not possibly be attempted for the recovery of reason; but the more moderate have been kept as quiet as possible, and some of the more furious have been sent away. The faculty who have attended the sick of this great hospital, and the committee who have superintended it, have done their duty faithfully: it is only to be lamented

lamented, that the evil, on the present system, appears to be irremediable. When the Poor-house shall be relieved from the Insane, the exertions of the respectable magistrates who are now so laudably employed in improving its regulations, will be more successful. Their attention being confined to the proper objects of this institution, they will then find it easier to extirpate vice, disorder, and guilty idleness, from this great family of the lowest and most ignorant class of society, to prepare the young for entering the world with habits of industry and sobriety, and to give comfort and quiet to the old, whose days of labour are past, and whose chief duty it is to prepare themselves for a future world.

2. It is said, that though an Asylum for Lunatics may be desirable, yet that sufficient accommodation may be found for them without erecting a building on purpose. Some have pointed to the present House of Correction as a proper place, and others mention, that cells may possibly be appropriated to them in the new Jail. That any plan which would separate the Lunatics from the general mass of the poor, would be an improvement on the present system, it is but fair to allow; but to each of these proposals there are strong objections. To the first, may be offered the objections already stated, against complicating the discipline of a
Poor-house

Poor-house with that of an institution for the Insane, with which it has no alliance. It may be urged likewise, that the present House of Correction, when vacated, will be wanted for a feverward to the Poor-house, to prevent the spreading of those contagions which, with every care, will sometimes be introduced into the building, and sometimes generated within its walls. And it may be added, that the House of Correction is very unfit for an Asylum for Lunatics from its structure and size, which a single glance will show to be inadequate to the purpose, and from its situation, which is exposed to idle curiosity and perpetual noise. On the other hand, the appropriation of any part of the new prison to the reception of the Insane, may be supposed a very crude notion. That great and scientific edifice is destined to a very different purpose; if it were not, no part of it could be adapted to incurables without considerable alterations; and who would seriously think of planting an institution for the recovery of reason, within the precincts of a jail?

The truth is, these proposals have proceeded from an imperfect notion of the design of a Lunatic Asylum. If its intention were merely to provide a prison-house for the Insane, where they might be hid from the sight of their friends 'till the

the grave should hide them for ever, such proposals would deserve consideration. But this is only one object of an Asylum, and that one the least important. It has the greater object of restoring reason itself, and such notions do not correspond with this high design.

3. But, while these schemes appear too narrow, another has been proposed that seems to run into the contrary extreme. Some warm supporters of the Lunatic Asylum, (particularly one gentleman, who has the power and the will to give it most generous assistance) are of opinion, that it ought to be a distinct institution, placed in the country, though near the town; where it may possess the advantage of the purest air, with a considerable space for the amusement of the patients in different exercises, and for their occupation in gardening and other innocent and healthful employments, when their minds are sufficiently calm to be engaged in this way. To this proposal the difficulty and the expence of carrying it into effect, are the only objections, for it cannot be denied, that it is, in the abstract, the best of all others.—Dr. Hunter of York, in a letter to the writer of this, gives a decided preference to this plan, and earnestly wishes, that every county in the kingdom would adopt it,—His opinion is the more to be regarded, because

because it is founded on actual experience, having himself been the principal agent in establishing an institution on this plan in his own county, to which he is the sole physician.—The YORK LUNATIC ASYLUM. But it is to be feared, that such a plan can only be successful when proposed as a county establishment, and it does not appear that Lancashire is likely to unite in a measure of this kind. In Manchester, a Lunatic Hospital connected with the Infirmary there has long flourished; and a similar establishment at Liverpool will probably answer every exigence that may arise, for many years to come. Should other districts of the county require in process of time similar establishments, they will doubtless, in this, as in other instances, follow the example of the leading towns. That the expence of a separate establishment in the country may be fairly computed, let us attend to a few obvious facts. In the Asylum at York, the patients are divided into eight classes, which pay according to eight different rates, regularly progressive, from six to twenty shillings a week. It may be presumed, that these rates defray the expence of maintainance, and that the sums raised for this institution by contribution, have been employed in the original purchase and improvement of the ground, in the building and furnishing the Asylum, and in the expence of a separate establishment. By the statement

ment published on the first of January, 1788, these
sums appear to be as follows. Raised

By Benefactions *l.* 9155

By Legacies 1718

By Collections 813

——In all *l.* 11,686.

There is no account of any accumulating fund ;
the York Asylum is adapted to sixty, or perhaps
seventy patients.

As the purchase of land, building, &c. cannot be expected to be cheaper in the vicinity of Liverpool than of York, a similar plan could not be executed here at a lower rate. And though we built a house for the reception of thirty patients only, yet, as the quantity of ground required, and the expences of the establishment, would not be much less, the sum of the expence could not be calculated at less than two thirds of that of the York Asylum.—But it seems scarcely to be expected, that eight thousand pounds should be procured for this purpose in the town and neighbourhood of Liverpool, where some of the leading men seem as yet indisposed to a Lunatic Asylum on the easiest and most economical plan. Should the opulence and public spirit of any individual, or of any combination of individuals convince the writer, that his doubts are ill founded, he will heartily adopt this more extended scheme, and promote it by the best of his humble endeavours.

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In the mean time the plan approved at the General Meeting, seems deserving of every encouragement, not only as the simplest, and least expensive, but as the only one likely to succeed, and as one that bids fair to accomplish all the more important objects.—By combining the Lunatic Asylum with the Infirmary, there will not only be an immense saving of expence in the building itself, but in the annual disbursements. The same offices, apothecary, and board of œconomy will serve both, beside other advantages; and for a third of eight thousand pounds, all that is wanted may possibly be obtained. To this union the same objections do not apply, as to a house for Lunatics in conjunction with the Poor-house. The discipline of an Infirmary, and of a Lunatic Asylum, have similar objects, and require the same habits, and nearly the same degree of watchfulness and attention. The Institutions themselves are closely allied in their nature; the first affords relief to diseases of the body, the second to diseases of the mind. That these are more nearly connected than is commonly imagined, it would be easy to show, if this were the proper place to enter on such discussions. Madness indeed can only be called a disease of the mind, because its most striking symptom is the derangement of the intellect.—The disorder, it is reasonable to suppose on every theory, is seated not in the agent but in the instrument of thought, and to borrow

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an expression from the letter of the enlightened physician already mentioned, a madman may be defined, "a man out of tune."

4. This subject must not however be dismissed without noticing an objection to the plan which has been adopted, that comes from a very respectable quarter. It has been supposed that the vicinity of the Lunatic Asylum to the Infirmary might be hurtful to this charity, from the patients in it being disturbed by the noise of the *Insane*.—As this objection is an important one, it has been particularly examined. Where experience can be had, it is always safest to have recourse to it, and this has been done in the present instance. On this particular point Dr. Hunter of York, Dr. Simmons, physician to St. Luke's Hospital, London, Dr. Eason, physician to the Infirmary and Lunatic Hospital at Manchester, Dr. Moncrieff of Bristol, and Dr. Cleghorn of Glasgow, have been consulted, and the writer of this has also had ample communication on the structure and œconomy of Lunatic establishments with Dr. Gilchrist of Dumfries. He has likewise, by the assistance of Mr. Christie of London, obtained a plan and an account of the Lunatic Hospital at Montrose. To detail the information contained in these letters would be tedious and needless; they are open to the inspection of any gentleman who may wish to peruse them, as well as a copy of the words in which

which the objection was stated. It will be sufficient to say, that on this point, the answers of such as have had experience, are clear and satisfactory. In the Dumfries Infirmary, Lunatics were at first confined on the ground floor, under the same roof with the other patients, but their numbers having increased, a separate building has been erected for them, which stands as a wing to the Hospital; and, as appears by a plan of the whole, at a distance from it of thirty feet. In this instance, the objection it is clear has never occurred. In Manchester the Lunatic Hospital is in close connection with the Infirmary, as those who have visited that town must have seen, yet here we have the express assertion of Dr. Eason, that no such inconvenience has been felt, and this account has been confirmed by Mr. Darbey, a very ingenious gentleman, who has seen the spot on which our Asylum is intended to be built, and who served the office of apothecary to the Manchester Infirmary for twenty years.

Mr. Christie and Dr. Simmons mention that this objection was started to the vicinity of St. Luke's with the Lying-in-hospital, but that it was over-ruled, and that experience has proved it to be merely hypothetical. Yet by the plan of St. Luke's, which the former has been so kind as to send, the distance between these buildings is only

forty feet. Between the Liverpool Infirmary and the projected Asylum, a distance of fifty yards, if necessary, may be obtained. If to these instances we add, that the trustees of Guy's Hospital, with all the experience of other institutions before them, and with a fund that is adequate to almost any expence, are about to erect a building in connexion with that Infirmary exactly upon our plan, it is hoped that the danger apprehended in our case, will no longer be feared.

Every new scheme must expect to meet with objections, and he whose judgment suggests them to him, does society a service in proposing them openly. If they are well founded, they may prevent an ill-advised project, if they are founded in part they may improve a hasty measure, and if they are altogether erroneous, they will serve to illustrate a wise purpose and a judicious scheme. Those gentlemen therefore who have openly opposed the plan of the committee, conceiving that a better might be executed, are to be applauded for withholding their assistance till their objections are in some degree removed; and such as have hesitated in their approbation of any measure of the kind, not perceiving the grounds on which it is supported, have done wisely to withhold their countenance for further information.—That the scheme of the committee does not comprehend

every possible advantage, it is but candid to allow ; but this is in no respect singular. We must not reject those advantages which are the best that can be obtained, because they are not as good as may be conceived, nor refuse a blessing, though it has some tincture of alloy. Bounded in our knowledge as well as in our power, we cannot expect that the work of our hand shall have the attribute of perfection.

To those who may be surprised that a measure which seems now so desirable, should not have been sooner carried into effect, the difference between the objects of this and of other charities, may be pointed out. The cries of poverty and of sickness will be heard, but Insanity alas ! cannot make its complaint !——Hence the victims of this disease have passed too much unregarded, and when they have been noticed, they have been thrust from the sight into prison-houses, whose secrets, if they could unfold them, might often “ harrow up the soul.” A late national distress has however forced the subject upon general attention ; the example of Liverpool, there is good reason to believe, will speedily be followed by several of the principal cities in the kingdom, and among the happy consequences of the issue of that calamity, future times will probably enumerate a more general provision, and a more humane treatment

treatment of this hapless class of our fellow-creatures.

The writer of this is not biassed in favour of the proposal for a Lunatic Asylum by his having been the author of the scheme. On the contrary, it was proposed originally without his knowledge; that honour belongs exclusively to others. But as there is a public, as well as a private duty attached to every station of life, when this proposal was brought forward, he conceived it came within the scope of his professional duties to form a judgment upon it on the best information in his power. —The grounds of this judgment he now lays before the public, and he has the satisfaction of thinking, that though this may appear an useless labour, it cannot possibly do harm. If the scheme is a good one, it cannot be too nicely examined: it is the character of truth and wisdom to appear more advantageous the more they are seen, it is for fraud and folly only to shrink from the light.

J. C.

P. S. Since writing the above, I have been favoured with a letter from Dr. Hunter of York, in which he has been so kind as to rectify one or two misapprehensions respecting the Asylum for lunatics there. He mentions, that instead of sixty or seventy, this building, with the last additions,

tions, will contain ninety patients, and that the sum total of its expence is something more than ten thousand pounds, fifteen hundred having been laid by for additions and repairs, a circumstance not mentioned in the reports of 1787, from which my statements were drawn.

Dr. Hunter is farther of opinion, that a separate building and establishment for thirty Lunatics, in the neighbourhood of this town, might be reared for a much smaller sum than what I have supposed, conceiving, no doubt, that the experience derived from the Asylum at York, which was in a great measure a new undertaking, would point out a less expensive method of attaining the objects in view.

Though I have argued on the supposition of a building for thirty patients only, yet, I am of opinion, that we ought not to erect one for less than forty, the grounds of which shall (if necessary) be laid before the public on some future occasion; and though in deference to this learned and respectable physician (whose kindness and liberality are worthy of every acknowledgement) his opinion is published as well as the facts he has offered, yet my predilection for the plan adopted, of connecting our Asylum with the Infirmary, as

the best that can be pursued in our circumstances, continues as firm as before.

In treating on this subject the following fact (which came very lately to my knowledge) ought to find a place, as it may serve to illustrate more strongly the propriety of some establishment for the Insane.

It appears that Mr. Howard visited Liverpool several years ago, and witnessing the situation of the Lunatics in our Poor-house, that he was impressed with our want of a separate building for their reception. This it should seem, dwelt on his mind, and some time afterwards he wrote a letter to the Mayor of that year (Mr. Pole) from Constantinople, recommending the erection of a Lunatic Asylum, and offering fifty pounds towards it, whenever it should be undertaken, a sum which his executors would be instructed to pay, on production of the letter, should his life be demanded of him before his return. Hence the name of *John Howard, of Cardington*, graces our subscription list. Mr. Howard has since been in Liverpool, and though he had much conversation with the writer of this, as well as with others, on the subject of our charitable Institutions, it does not appear that he took any notice of his own remarkable offer. He learnt however that the scheme

scheme for erecting a Lunatic Asylum, had been brought forward by a public-spirited gentleman, and though deferred for some time, that it was likely in the course of a few years to be carried into effect. Mr. Howard is again gone abroad, and should he live to return and revisit Liverpool, it is hoped that in this particular he will not be disappointed. But this is a pleasure which it is feared he may never enjoy.*

Having awakened the powers of reason, and the true spirit of charity throughout the nations of Europe, he is now attempting to diffuse them among the disciples of Mahomet. While the sovereigns of Russia and Germany, are carrying devastation and slaughter along the coasts of the Euxine and the shores of the Archipelago, this *Prophet of Mercy*, approaches the benighted followers of the Crescent, from another quarter, with a mission of peace and love. He was last heard of from Petersburg. Thence passing through Moscow, he purposed to enter the Turkish empire eastward of the sea of Azof, to avoid the storms of war. The rout he has marked out crosses the mountains of Circassia, and passes along the shores of the Caspian into Persia and

* He died on this journey at Cherson.

Armenia. His pilgrimage will then extend across Arabia Petrea, and through the Isthmus of Suez into the continent of Africa. If life is granted him, he will traverse the nations that inhabit the southern shore of the Mediterranean, and passing into Europe by the streights of Gibraltar, return by Spain and France to England. To this singular tour he has devoted three years, and he himself, it is said, has little expectation of living to go through it. It is most probable, therefore, that we shall not see him again.—But no matter—wherever he finds a grave, the spot will be hallowed, and his name consecrated in the admiration of posterity.

Quo nihil majus, meliusve terris

Fata donavere, bonique Divi ;

Nec dabunt, quamvis redeant in aurum

Tempora priscum.

HOR.

JA. CURRIE.

Liverpool, 12th November, 1789.

N. B. The Lunatic Asylum was completed in the year 1790, nearly on the plan recommended above. It has accommodations for sixty-four patients. No inconvenience has been ever experienced from its vicinity to the Infirmary.

J. C.

May 7th, 1804.

Preface to the Letter to Dr. Beddoes.

HAVING in my dedication to Sir Joseph Banks mentioned the use of the Nitric Acid, as a remedy in *Lucs Venerca*, I think it right to give the following letter to Dr. Beddoes, as containing the result of my trials of it at the time the letter was written—my subsequent experience is of the same mixed nature.

I have not used the Nitric Acid as a remedy in fever—some of my friends have tried it in that disease, and thought it salutary.

May 8th, 1804.

*Letter to Dr. Beddoes, on the Nitric Acid.**Liverpool, 1st October, 1798.*

Dear Sir,

I AM truly sorry I have occasioned you the trouble of writing so often to me, by neglecting to comply with your wishes respecting the nitric acid; but the truth is, my experience of its effects in lues is not so extensive, or so uniform, as to enable me to speak with confidence on a subject where accurate conclusions appear to be so difficult. Nevertheless, since you desire it, I will give you a short account of what I have observed.

I began to use the nitric acid in lues, at our hospital, in the beginning of 1797. In the two first cases, there were ulcerations on the penis, and open buboes in the groin, but no decided evidence of the system being affected; and the disease was in each case of less than three months standing. In the third, the disease had been in the habit upwards of a year; the surface was

covered with venereal eruptions; the throat had been affected, and the glands of the neck, on each side, had been indurated, and were in a state of open ulceration. The patient had undergone a course of mercury in the hospital; but after pushing it as far as her system would bear, she had been discharged, about six weeks before, with little or no amendment in her symptoms. The last six weeks she had been in the country, on a milk diet, and her health was somewhat recruited. I paid much attention to these three cases, and have minutes by me respecting them, of considerable extent. The two first were males.

Each of the men took a pint of water daily, gratefully acidulated with the nitric acid; a drachm being at first used in each pint, and afterwards a drachm and a half; but this last proportion appearing to affect the bowels by griping, the original proportion was returned to, and the patients took a pint and a half of the acidulated water daily; i. e. a drachm and a half of the acid, as already mentioned. In the case of the female we never exceeded a drachm, her bowels being very irritable,

In five weeks, every symptom of disease in the two men was gone; and I presented them to the Board, as remarkable instances of lues being cured without

without the use of mercury. They attended at my house weekly, for some time; but, being sailors, they went afterwards to sea, and I have never heard of them since.

In the female, the same happy progress continued for nearly a month; the eruption on the skin diminished, the nocturnal pains in the head and limbs went off, and the ulcerations in the neck assumed a healing appearance; her general health, also, improved rapidly. But at this period, her progress towards a cure stopped; and though we persisted in the acid some time, it did not recommence. It was therefore, at the end of seven weeks abandoned, and recourse had again to mercury, but in small doses, gradually increasing them, however, till ptyalism commenced. At first, there were indications of benefit from this new course of mercury; but these speedily failed; and her general health suffering severely, we were once more compelled to abandon it; the ulcerations in the neck having, during its use, evidently spread and become more morbid. The nitric acid was had recourse to, as before. During the second course of the acid, her health again improved, and the venereal symptoms again appeared to give way. But, after a few weeks, these favourable indications failed us; we abandoned the acid, and resorted to mercury once more. It would be tedious

tedious to particularize farther. With the mercury, sarsaparilla, mezereon, and opium, were successively combined; but in vain. Her health giving way, the sores enlarged, and once more we returned to the acid. At length we combined the acid with mercury, in what are called alterant doses, and with evident benefit. At the end of eight months, however, the ulcerations continued, though much diminished. Despairing of any farther benefit from these combined powers, we abandoned them altogether, and after a proper interval, put the patient on a course of the mineral solution of De Valangin, (from which, in obstinate venereal affections, I had before seen extraordinary effects) and during this course the ulcerations speedily healed, and the cure of the patient became complete, the treatment having occupied a space of upwards of ten months. In obstinate cases of this kind, it is usual to suppose, that some scrofulous, or other taint, has combined with the venereal virus, and this may have been the fact in the present instance; the result will however, afford encouragement in similar situations.

The encouragement arising from the three cases just mentioned, led me to try the nitric acid in a variety of other cases. In some of these, my success has apparently been complete; in others, there has been evident benefit without a perfect cure;

cure; and in others, it has seemed to fail entirely. It is not a little curious, that in some of the cases in which I have succeeded, the symptoms were what are called secondary, and the disease in its most rooted and obstinate state. One of my patients, whose name is Elkins, has drawn out his own case, the particulars of which are shortly these:—About four years ago he was affected by lues, with the usual symptoms, for which he underwent a course of mercury, and was supposed cured. In about nine months afterwards, however, the disease appeared in his throat, and in obstinate pains in his head, &c. He was again salivated, and with similar good effects. Twelve months after this, having been for a considerable time subject to what was supposed to be rheumatism, the disease appeared again, and resisted the long-continued and repeated use of mercury, under a practitioner here of the first eminence. He was at length obliged to abandon it, having been reduced to a state of extreme weakness. About three months after this, he was admitted a patient into our hospital, and under my care. At this time he had a thickening of the pericranium in two different places, the most severe pains, especially in the night, in the bones of his head, arms, and legs, and a large increasing node on the right tibia. All his symptoms were at this time increasing; and having taken so much mercury in vain, he

he was in a state of extreme despondence and depression.

We prescribed the nitric acid, and his sufferings abated from the third day; and being continued, the thickening of the pericranium and the node of the tibia entirely disappeared, with all his other symptoms. He took the nitric acid, in all, to the quantity of eight ounces in eight gallons of water, which he drank in sixty days.

Elkins has been nearly a year discharged, and has never had any return of his complaints. This case has made some noise, and I have endeavoured to attract the attention of several of my brethren to it, as decisive of the influence of the acid in this destructive disease.

On the other hand, there are a still more considerable number of cases, in which the acid has entirely failed me, or produced only partial benefit; and at present, though I always ordered it internally, with mercurial inunctions on the skin, I do not trust the cure to it alone *in the first instance*. Combined with mercury in this way, the constitution seems to support the action of the metal better, and the cure to be accomplished more safely and more speedily. In several instances where, after a course of the nitric acid, it has
been

been thought adviseable to have recourse to mercury, a very small quantity of the ointment, in one case two drachms only, has produced complete ptyalism. This has occurred so frequently, that I do not think the conjunction accidental.

The nitric acid has never been pushed by me to the extent in which it has been used by others ; in many of the cases, in which it has apparently failed, I cannot pretend to say, that it would not have succeeded, if pushed to a greater length ; but I have not thought it proper to carry it to an extent injurious to the stomach or bowels, while the salutary effects of mercurial inunction remained untried. In the quantities in which I have prescribed it, it has been uniformly salutary to the constitution, in this respect its action contrasting very happily with that of mercury. In the cases in which it has apparently succeeded (in my hands) in the cure of lues, I have not known a relapse to take place ; but as the patients have been chiefly seafaring persons, it is not in my power to trace their history subsequent to our parting. In one case of the primary disease, in the hospital of the 20th regiment, the assistant surgeon, who thought it had effected a complete cure, found the disease break out in the throat, at the distance of four months, and finally removed it by mercury.

I have

I have experience of the effects of the nitric acid in complaints of the stomach, hypochondriasis, asthma, and some other diseases, as well as in hepatitis; but as your inquiries are directed to its agency in a single disease—lues—I forbear to enter on other points.

But you will ask whether I can mark, by any particular effects, the circumstances attending its salutary operation in lues?—I think I can. In the cases in which it succeeded, it evidently irritated the system in the following respects:

1. The gums were always affected with tenderness and redness, and the action of the salivary glands increased. This affection may indeed be considered as depending on its local action; for it took place in one case, where, for the sake of the experiment, the acid was taken into the mouth, but not swallowed. This state of the gums, &c. was not attended by foetor, as during the action of mercury, neither did it increase as the acid was continued, but in a little while disappeared. This affection of the gums and salivary glands did not always appear in the cases where the acid failed.

2. In every case in which it succeeded or operated beneficially, there was a considerable increase

of urine, and this discharge became turbid ; sometimes of a whitish, and sometimes of a brownish hue ; it amounted to about eight pounds in the twenty-four hours. But the discharge of urine did not go on increasing under the continued use of the acid ; on the contrary, it ceased like the increased discharge of saliva. The affection of the kidneys seemed to precede the affection of the salivary glands ; but both the one and the other occurred by the fifth day at latest.

3. The patients had their appetites improved, and felt a greater alacrity of spirits.

4. In all of them the pulse was rendered more frequent, and the animal heat towards evening increased from one to two degrees, which in the night was generally carried off by more or less of sensible perspiration.

I think that these symptoms have not appeared at all, or not in combination, where the acid has seemed to be inert.

But I wish to speak with the diffidence becoming my imperfect experience ; and I would not willingly have spoken at all in this stage of the inquiry, had not there appeared a danger of the attention of medical men being wholly withdrawn from

from the investigation of a subject, which, though difficult, seems to me not only curious in itself, but likely to produce important consequences to the healing art. To assist in preventing this, I am willing to offer a testimony, which is certainly imperfect, and which in some respects may ultimately be found erroneous.

You are welcome to make what use of it you please.

I am, dear Sir,

Yours with much esteem,

J. CURRIE.

THE END.

J. M. Adams

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